MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXIII. *May* 9, 1873. No. 7.

PROFESSOR CAYLEY, President, in the Chair.*

Sidney Waters, Esq., Oakhurst Lodge, Tufnell Park, and Wm. Hy. Hermah, Wiltshire Road, Brixton,

were balloted for and duly elected Fellows of the Society.

Note addressed to the Astronomers of the United States, on the subject of the approaching Transit of Venus, at the suggestion of a distinguished European Astronomer. By R. A. Proctor, B.A., Cambridge.

During the course of correspondence which I have had on the subject of the approaching transit of *Venus* with one of the most eminent astronomers of this or any country, the idea has been suggested to me by him that advantage would result to science if an appeal were made to America to furnish forth expeditions to the Antarctic and sub-Antarctic regions for the purpose of making those Southern observations without which the Northern observations at Halleyan stations will be altogether useless. "America has frequently shown great interest," he remarks, "in southern exploration, while she also possesses good telescopes and competent observers to use them. Let both countries do their best, and science, which is of no country, will benefit all the more."

I gladly act upon this suggestion, remarking only that while

* Prof. J. C. Adams, F.R.S., was in the Chair at the April Meeting, not Prof. Cayley, as announced in the last Monthly Number.

I recognise the abstract justice of the proposition that science is of no country, I cannot altogether free myself from the hope which I have long entertained and expressed, that in the struggle to advance scientific knowledge this country may worthily maintain her position.

My appeal to America is based on considerations which I have already urged elsewhere. If the great problem for which the coming transits are to be observed is really important to science (on which point no one, I suppose, can entertain any question), then the circumstances to which I advert are of no light significance.

We need not closely inquire whether one interpretation or another of the peculiarities of internal contact be correct. It is not a question whether one or another method have some slight or even considerable advantage. Nor again is it a question whether this or that Antarctic or sub-Antarctic station can be occupied or not.

What I urge on our American fellow-students of astronomy, as I have urged and still urge at home, is the adoption of arrangements for occupying *many* stations in the Southern hemisphere, lest the whole matter end in failure, or in a success so partial as to compare very unfavourably with what was accomplished in 1769. I mention as a mere detail, that the distinguished astronomer whose advice I am following, altogether concurs in my opinion that the *duration* of the transit should be observed at as many favourably situated Southern stations as possible. And every one who considers what Russia, America, and Germany are preparing to do at northern stations,—no less than sixteen of which are to be occupied where durations can be favourably observed,—must feel how necessary it is to call attention to the fact that at present there are but four or five third-class Southern stations for observing durations, and only one first-class station. But though I cannot but dwell on this fact, fortified as it is by the circumstance that the photographic and direct methods are equally ill provided for, I do not rest my appeal on details of the sort. It is the risk of absolute failure, and the certainty that the Southern stations hitherto provided for are insufficient in number, to which I earnestly invite the attention of American astronomers.*

The region to be occupied is indicated in the chart which appears in the present monthly number. Of Antarctic stations there are Enderby Land, Sabrina Land, Adélie Land, and Possession Island, as well as the whole region (including these places) surveyed by Wilkes, Ross, D'Urville, Billingshausen, and others. Of sub-Antarctic stations there are Kemp Island, Macdonald Islands, Emerald Island, the Crozets, Royal Co. Island,

* Granting even that fine weather prevailed at each of the few Southern stations, the probable error of the resulting determination of the solar parallax must necessarily be enhanced when the Southern stations are so few compared with the Northern.

and others, very uninviting beyond all question, but doubtless including several accessible stations. Unfortunately there is now little time for preliminary survey during the Antarctic summer of 1873–74; but if such survey cannot be undertaken, then in the autumn (or Antarctic spring) of 1874, two or three ships (preferably steam ships) might proceed direct to the region indicated, each conveying two or three well-provided observing parties, and combining reconnaissance with the occupation of stations as they were successively selected.

That it is perfectly in the power of this country and America to ensure the requisite number of Southern observations of the coming transit, I am satisfied. There is, it is true, no time for delay. Energy and skill will be wanted; but they have never been looked for in vain in such circumstances. The expeditions which would have to be made would be no pleasure-parties, nor would they be free from difficulties and dangers sufficient to tax the courage even of British and American seamen. But these very considerations encourage the students of science in both countries to believe that the required effort will be made. That it should be made, if failure is to be averted, does not seem to me to be open to the slightest question.

List of Stations selected for Observation of the Transit of Venus by Russian Astronomers. By M. Otto Struve.

(*Communicated by the Astronomer Royal.*)

The following is a corrected list of stations for the observation of the Transit of *Venus*, as fixed at the General Meeting of the Russian Committee held on March 22, 1873:—

Station.	Latitude.	Longitude E. of Greenwich.	Instrument to be employed.	Proportion of Clear Sky in beginning of December.
	° ′	h m		Per Cent.
1. Nakhodka	42 48	8 51·4	6-in. refractor	80–85
2. Port Possiet	42 40	8 43·0	Photoheliograph; 4-in. telescope	80–85
3. Hanka	45 4	8 50·0	Heliometer	80–85
4. Busse	46 24	9 15·0	3-in. telescope	unknown
5. Jeddo	35 36	9 19·0	4-in. telescope	unknown
6. Pekin	39 54	7 45·7	4-in. telescope	80
7. Habarowka	48 16	8 58·8	3-in. telescope	80–85
8. Nertschinsk	51 18	7 58·5	Heliometer; 4-in. telescope	70–80
9. Tschita	52 1	7 34·0	4-in. telescope	90
10. Kiakhta	50 20	7 6 7	Photoheliograph; 4-in. telescope	70–80
11. Blagoweschtschensk	50 15	8 30·5	Heliometer; 4-in. telescope	80–85

Station.	Latitude.	Longitude E. of Greenwich.	Instrument to be employed.	Proportion of Clear Sky in beginning of December.
	° ′	h m		Per Cent.
12. Omsk	56 30	5 40·3	3-in. telescope	below 50
13. Taschkent	41 19	4 37·3	6-in. refractor	60
14. Fort Perowski	44 51	4 21·9	4-in. refractor	80
15. Fort Uralsk	51 11	3 26·4	6-in. refractor	60
16. Orenburg	51 45	3 40·5	3-in. telescope	below 50
17. Krasnowodsk	40 0	3 32·0	Photoheliograph; 4-in. telescope	70–80
18. Aschuradeh	36 54	3 35·8	6-in. refractor	80–90
19. Naktritchevan	39 12	3 1·6	4-in. refractor	90
20. Erivan	40 10	2 58·1	6-in. telescope	90
21. Tiflis	41 42	2 59·3	4-in. telescope	below 50
22. Jalta	44 30	2 16·7	4-in. telescope	60
23. Kertch	45 21	2 25·9	3-in. telescope	60
24. Kazan	55 47	3 16·5	9-in. refractor	
25. Nicolaiew	46 58	2 7·9	4-in. telescope	
26. Odessa	46 29	2 3·0	6-in. refractor	
27. Kharkow	50 0	2 24·9	4 in. refractor	

One station on the Russian territory will probably be occupied by American astronomers, namely,—

	° ′	h m		
28. Wladiwostok	43 7	8 47·7	American Photo-heliograph.	80–85

The three stations for which the chances of clear sky are below 0·5, have been introduced for the reason that probably we shall have there good observers, engaged in other pursuits, whom we have only to provide with telescopes to enable them to make good contact observations, in case it should be clear. The same remark relates to the stations Jeddo, Pekin, and Busse. At the four fixed observatories, Kazan, Nicolaiew, Odessa, and Kharkow, the conditions are on the whole very unfavourable for the observation. Concerning the temperature to be expected at the time of transit, we have only the following estimations:—

Nakhodka, Possiet, Busse, Wladiwostok, Hanka	about 32° Fahrenheit,
Nertschinsk, Tschita, Blagoweschtschensk, Habarowka, Kiakhta	0
Pekin	32
Omsk	10
Taschkent	40
Uralsk, Perowski	25
Orenburg	20
Krasnowodsk, Aschuradeh	40
Naktritchevan, Erivan	50
Tiflis, Jalta, Kertch	45

At Nertschinsk and the other stations of Eastern Siberia the cold is rather strong, but, according to Professor Schwarz, who has spent ten years in those parts, that cold is commonly accompanied with a complete calm and dry air, and thereby it is not unpleasant for astronomical observations. With this conviction, Prof. Schwarz has selected for himself the station of greatest cold, Nertschinsk.

For nearly all the stations the observers are already designed, and will practise themselves this summer at Pulkowa in the use of their instruments. All the telescopes will have an equatoreal mounting; those designed as refractors are provided with clockwork and micrometrical apparatus for measuring the cusps, or the distances from the Sun's limb. Personal equations will be determined by help of an artificial transit apparatus.

The telegraphic longitude determinations through Siberia will be executed in the course of the next two years. The stations selected for that purpose comprehend several *Venus* stations, namely, Nertschinsk, Blagoweschtschensk, Habarowka, Wladiwostok, and Taschkent. The other stations can be easily joined with these by chronometric operations. For the stations near the Caspian and Black Seas, the longitudes are already known with sufficient accuracy. All the observers will be provided with instruments for the determination of time and latitude.

Pulkowa, 1873, *April* 6.

Reply to the Remarks of Mr. Proctor upon the Position of the Lunar Atmosphere, when in a state of Equilibrium. By John J. Plummer, Esq.

(*Communicated by the Rev. Professor Farrar, D.D.*)

In discussing the possibility of our atmosphere enveloping that portion of the lunar surface remote from the Earth, which, being brought to the limb by libration, is the cause of the projection of stars within the lunar periphery, I had taken as a basis the investigations of M. Hansen, published in the *Memoirs of the Royal Astronomical Society*, vol. xxiv. If the correctness of those deductions be denied, my theory at once falls to the ground with them, but if, on the contrary, it be admitted, I can by no means concur in Mr. Proctor's argument. In the first place, I fail to find in M. Hansen's memoir any allusion to the irregular figure mentioned by Mr. Proctor, as representing that of the Moon on Hansen's supposition, nor am I clear upon what principles the centre of symmetry of such a mass would be found. It is true that M. Hansen considers it unlikely that the Moon is exactly spherical, yet he is careful to add, that it is quite possible

to imagine the laws of its density, by which the same results may be brought about, though the figure be that of a perfect sphere. He prefers to regard our satellite as an ellipsoid with three different axes, and by doing so appears to me to take the only philosophical view of the question possible. We may evidently regard the lunar atmosphere as forming a series of hollow spheres of gradually diminishing density, all having a common centre (viz. the centre of gravity of the entire mass), and broken only where solid matter protrudes through them. The sole question at issue would therefore appear to be, What is the true form of the Moon? for accepting Hansen's theory the centre of gravity is known. Now I conceive that it must be either a sphere or an ellipsoid differing but little from a sphere, because we are unacquainted with any natural laws which could tend to produce an irregular meniscus figure, and I contend that this supposition is not at all at variance with Hansen.

Now, whichever of these regular figures it may be, provided that, if the latter, the ellipticity is not considerable, the fact of the centre of gravity being as much as 36·66 miles further removed from us than the centre of figure, will cause the greatest depth of atmosphere to lie upon the centre of the invisible hemisphere, while the visible disk at mean libration, even to the margin or limb, will be entirely denuded, if the total amount is limited. This is the conclusion arrived at by M. Hansen himself, and clearly shows that he also regards the deviation from the spherical form as being but slight. It may be remarked that this assumption is the more probable, since Newton has calculated that the effect of the Earth's gravity upon a fluid Moon would only elongate the axis in the direction of the radius vector by the insignificant amount of 186 feet, and it does not appear possible to imagine any other cause capable of producing any elongation whatever. It is a curious fact, and one that has not been previously noted, that though there are several methods by which the existence or non-existence of a lunar atmosphere can be demonstrated, not one of them has invariably given negative results.*

Although I am not interested in refuting Mr. Proctor's remarks upon the variation of the Earth's atmosphere, I must

* In case any one may desire to look for evidence of the lunar atmosphere during the forthcoming partial solar eclipse I have calculated that the geocentric libration of the Moon's disk will amount to exactly 5° at the mean time of conjunction of the Sun and Moon, and that the point of the lunar disk where it attains this maximum amount, is situated at 105° from the north point, measured towards the west for direct image. Towards the latter part of the eclipse it is therefore possible that a bright line of light may be traced upon the solar disk near the western cusp, but the amount of the libration is not favourable to this being very distinctly marked. It is further necessary to remember that at no time during the eclipse does the point of maximum libration actually come within the solar margin, although at Durham and at places still further north it approaches it very nearly, at about half an hour previous to the final contact.

equally disagree from him on this point also. If we disregard the Earth's rotation, the waters of the ocean will only arrive at the position of equilibrium when every point on their surface is equidistant from the centre of gravity of the Earth, which may or may not be centrically situated with respect to the general surface of the solid portion of the globe. If, therefore, any part is to be regarded as a meniscus, it would seem to be rather the high plateaux and mountain-chains of the northern hemisphere. But in determining the shallowness or depth of the atmosphere reference is always made to the sea-level, or, since the atmosphere is a hollow globe, concentric with the sphere of the ocean, its depth must be everywhere the same measured from that level. Of course the diurnal rotation modifies the fact, though it does not render the argument in any degree inapplicable.

The Observatory, Durham,
May 5th, 1873.

Note on Mr. Plummer's Reply. By Richard A. Proctor, B.A., Cambridge.

In commenting on a point touched upon in Mr. Plummer's former interesting paper, it was not my purpose to attack his theory. I have had no opportunity of forming a theory respecting the projection of stars within the limb. As I understand that experienced observers have recognised the phenomenon (though very seldom), I conclude it is not merely subjective. The fact that observers (also of great experience) have never recognised it, seems to point to another theory than Mr. Plummer's, since one can hardly imagine that these astronomers have not often observed occultations occurring near points of the limb most affected by libration acting towards the Earth. A sufficient explanation seems found in the possible existence of deep clefts: and it appears to me that whereas a star seen through a lunar ravine too narrow to be telescopically discerned in any other way, *would* seem to be projected within the limb, a star affected by refraction would *not* so appear, but would be raised *to* the limb by refraction, gradually fading from view, but throughout appearing on the limb. I must confess that this view of the matter would not explain the systematic occurrence of projection near points of maximum libration; and it would be a point of great interest to observe as many occultations as possible near such points, so as to have a greater number of instances on which to base an opinion. I imagine it would not be found difficult to supply a list of cases of projection occurring at times of mean libration, or even at points of maximum libration *from* the Earth. Some observations which I remember to have read about appear to me

to force upon us the conclusion that the phenomenon either depends on peculiarities of our own atmosphere, or on lunar irregularities. I would, for instance, cite the following passage from Smyth's *Celestial Cycle*,—

"One of the most remarkable projections of a star on the Moon's disk which I ever observed, was that recorded in the fifth volume of the Astronomical Society's *Memoirs*, p. 363, of 119 *Tauri*, on the 18th December, 1831. On that occasion the night was beautiful, the Moon nearly full, and the telescope adjusted to the star, which passed over the lunar disk" (meaning, I suppose, along the upper limb), "and did not disappear till it arrived between two protuberances on the Moon's bright edge. This was also noted by Mr. Snow, p. 373, of the same volume; but Sir James South saw nothing remarkable, although in a few minutes afterwards he observed the star 120 *Tauri* perform a similar feat." "Such anomalies," adds Smyth, "are truly singular." I cannot but think, however, that they are to be expected as a natural consequence of the unevenness which certainly characterises certain parts of the lunar limb. Many irregularities on the limb must be so minute as to be concealed through the effects of irradiation; and a very slight difference in the position of two observers would suffice to render the observed phenomena at their two stations altogether different.

But I am not concerned to discuss this point at length, having no theory of my own upon the matter, and only an opinion (which may be erroneous) respecting Mr. Plummer's theory.

Turning, however, to the point on which I touched in my former note, I have to remark that my comments related to a more recent phase of the discussion about Hansen's views than that referred to by Mr. Plummer. In 1862 M. Gussew, Director of the Imperial Observatory at Wilna, applied to photographs taken by Dr. De La Rue, processes of careful measurement, which led to the following results:—"The outer parts of the visible lunar disk belong to a sphere having a radius of 1082 miles, the central parts to a sphere having a radius of 1063 miles; the centre of the smaller sphere is about 79 miles nearer to us than the centre of the larger; the line joining the centres is inclined at an angle of about 5° to the line from the Earth at the epoch of mean libration; thus the central part of the Moon's disk is about 60 miles nearer to us than it would be if the Moon were a sphere of the dimensions indicated by the disk's outline." According to these results the Moon would be egg-shaped—at least the visible portion would be shaped like the smaller end of an egg (not differing greatly, however, from a sphere in general shape.) The centre of gravity of the Moon, if of such a figure, and of uniform density, would be farther away than the centre of the diameter directed towards the Earth. It was to this view of the subject, on the probability of which I make no

comment, that my remarks related. The following passage from a letter of Sir John Herschel's, bearing date May 11, 1870, will be interesting to Mr. Plummer. It relates to my objections to the theory of an atmosphere on the unseen part of the Moon :— "I believe your criticism on my theory" (I had not been aware that Sir John Herschel held the theory when I indicated objections to it) "is just. As for the water there can be no hesitation, —all the lunar oceans might readily subside into a basin 40 miles deep, and a hemisphere broad, without a chance of being seen by libration, but with an atmosphere anything like the Earth's, and only one-sixth the coercive power of gravity, some trace might certainly be expected to crop out at the border."

The slow change of density with elevation, owing to the small power of gravity at the Moon's surface, is a point which must be carefully considered in inquiries relating to a lunar atmosphere. It introduces an objection (fatal, I fear) to Mr. Plummer's ingenious theory.

There are some points in Mr. Plummer's reply which appear to me to be open to exception, but which I have not leisure to discuss so fully as I should wish. For instance, Newton's views as to the figure which would be assumed by a fluid moon are quite inconsistent with the hypothesis of a centre of gravity displaced $36\frac{1}{2}$ miles from the centre of figure, as will be manifest to any one who examines Newton's reasoning. Next, the laws of equilibrium for fluids and liquids do not lead to so simple a result, in the case of an ellipsoid with an eccentric C. G., as Mr. Plummer indicates. Again, regarding the Earth (non-rotating) as a spheroid, with a displaced C. G., a fluid would not be in equilibrium when every point of its surface was equidistant from the Earth's centre of gravity. Mr. Plummer also appears to me to use the word *meniscus* with a more restricted significance than is, I think, usual. I may remark, lastly, that apart from all theory, the depth of the atmosphere, measured from the sea-level, is not "everywhere the same," but markedly less in high southern latitudes than in corresponding northern latitudes,—the difference being equivalent to that due to half a mile of elevation above the sea-level.

Note explanatory of a Stereographic Chart of the Transit of Venus in 1874. By R. A. Proctor, B.A., Cambridge.

This chart is intended to illustrate a relation to which Professor Adams called attention at the last meeting of this Society. If we disregard the rotation of the Earth during ingress or egress, and also neglect the curvature of *Venus's* shadow-cone where it crosses the Earth, it is manifest that the points where the shadow touches the Earth first and last at ingress, or at egress, are respectively antipodal, and may be regarded as the

poles of a series of circles of equal acceleration and retardation, of equal value therefore for applying Delisle's method. Moreover, these circles manifestly indicate a value proportional directly to their distance from the plane of the great circle having the before-mentioned points as poles. The intersections of these circles indicate points of a particular value for Halley's method, the excess or defect of duration being (i) the sum of the corresponding accelerations or retardations where each of two intersecting circles indicates a time difference of the same kind, or (ii) the excess of acceleration over retardation, or of retardation over acceleration where the time differences are of different kinds. It is readily seen that if points of equal value for Halley's method are connected, the connecting curves are a series of circles, having as poles the points midway between the poles of maximum retardation and those of maximum acceleration. Moreover, these circles, like those of equal value for Delisle's method, indicate values directly proportional to their distance from the plane of the great circle having these mid-points as poles.

In my chart the several curves corresponding to these circles have been drawn; but *all the corrections depending on the Earth's rotation during ingress and egress, and on the curvature of the shadow-cone, have been carefully taken into account.*

The dotted red* curves are those indicating the *loci* of points of equal value for Delisle's method, and the red curves indicate the *loci* of points of equal value for Halley's method. The actual accelerations or retardations in minutes, and the differences of duration, have been indicated in red letters.

The interpretation of the chart, and the regions indicated as suitable for the various methods proposed to be employed, will be manifest even on a very slight inspection of the chart.

Note upon the Figure and Diameter of Venus.

By John J. Plummer, Esq.

(*Communicated by the Rev. Professor Farrar, D.D.*)

The transit of *Venus* across the Sun's disk affords, as is perfectly well known, the best opportunity for determining the ellipticity of figure of that planet, and it has been very often looked forward to, in order to settle this question as well as the more

* As a rule, I prefer to have only one printing in a chart of this sort, where every line has been laid down with scrupulous accuracy, because the coloured lines may not be printed quite correctly. But in the present case, the map would have been overcrowded by black lines, unless red had been used. To learn the amount of error in 'registering,' it is only necessary to compare the red and black impressions of the small cross lines indicating the points of maximum acceleration and retardation.

important one of the solar parallax, for which such extensive preparations are in progress. It will be thought, perhaps, that the minor problem may fairly be left to the observation of any astronomer possessed of the requisite means for solving it satisfactorily, but it may not be out of place to draw attention to the fact thus early, as some preparation is necessary even in this matter. Since it is not at all probable that the ellipticity to be determined is considerable, the observation will be a delicate one, and will be best undertaken by the larger instruments of the fixed observatories: the five expeditions, which are to be sent out, should certainly be relieved of the task and of the responsibility. There are unquestionably a sufficient number of English observatories situated in that portion of the globe from which the transit will be visible, but it is doubtful whether they are furnished with the best means of measuring planetary diameters. None, I believe, have large heliometers, and it would be impossible now to provide them with such, but Airy's double-image micrometer would probably give us reliable results, and could be readily supplied to those observatories at present without this most useful and accurate instrument. It should be employed with the greatest available aperture, especially as there is now no difficulty in finding a variety of means for reducing the intensity of the solar light and heat. But whatever plan was adopted for this purpose, the irradiation correction would be proportionally diminished, and most trustworthy measures of the real dimensions of the planet arrived at, apart from the values of the diameter which the times of transit across the limbs of the Sun will yield.

The probable minuteness of the ellipticity would further render it necessary to have the position of the planet's equator computed beforehand, and hitherto this has not been given in any ephemeris. As far as our present knowledge of this element goes, there is really no evidence that the ellipticity of the disk of *Venus* may not be as great as that of *Mars* or *Mercury*, when it would possibly effect the determination of the amount of the solar parallax, and though this is unlikely, it can only be necessary to call attention to the question for it to be thoroughly arranged for and investigated. My excuse must be, that it seemed liable to be overlooked or forgotten amid the din of preparation for the much more important parallax observations.

I have long been of opinion that the diameter of *Venus* used in the *Nautical Almanac* (viz. 16″·61 at the unit of distance), and which is the value determined by Encke from the transit of 1791, is too small. In 1868 I made a series of measures with the double-image micrometer for the purpose of shedding some light upon this point. These measures have never been published in consequence of their indicating a considerably larger co-efficient for irradiation than the similar observations of Mr. Main, in the *Mem. Roy. Ast. Soc.* vol. xxv. p. 46, but I am now engaged in repeating the observations with greater care and precaution, and hope to publish the results shortly. As far as these are at present

obtained, they appear to confirm the previous measures. The diameter found from twenty-eight observations, each consisting of twelve contacts, and made upon separate days between the dates March 19th and June 20th, 1868, was 17·695 at the mean distance of the Earth from the Sun; and though I cannot place much weight upon the result, as being my first essay with the double-image micrometer, it only slightly exceeds Mr. Main's final determination. As there seems, therefore, some reason to doubt whether even the diameter found by Mr. Stone from the Greenwich meridian observations (16″·944), to which Mr. Dunkin refers in the last number of the *Monthly Notices*, is large enough, I trust the measurements upon which I am engaged may have a special interest at the present conjuncture.

Durham Observatory, April 24th, 1873.

Observations of the Planet Venus in 1873. By T. G. Elger, Esq.

I beg to lay before the Society the following observations of the planet *Venus* made during the last three months with an achromatic of 4-inch aperture by Cooke.

Whenever the weather permitted, I observed the planet a few hours before sunset, as I always find that the markings on the disk, and the form of the terminator are seen to much greater advantage in broad daylight than at night.

The twenty-five drawings which accompany this paper* are "transfers" from sketches made at the telescope.

Jan. 2nd, 1873. 4^h to $4^h\ 30^m$. I noticed a conspicuous marking which extended from the N. limb of the planet through the centre of the illuminated disk; its shape and position are shown in Fig. 1. The S. junction of the terminator with the limb was evidently rounded off, while in the vicinity of the N. limb the terminator was slightly concave. (*a*. Fig. 1.)

At 6^h on the same day, the N. half the terminator formed a perfect "Ogee" curve. (Fig. 2.)

Jan. 3rd. 3^h to 4^h. No irregularities in the shape of the terminator were remarked, except the evident rounding off noticed on the previous day. A faint marking, very similar in shape to that observed at 4^h on January 2nd, was seen. (Fig. 3.)

Jan. 5th. 3^h to $3^h\ 30^m$. An irregular-shaped marking was distinctly visible, in spite of the unfavourable state of the atmosphere. (Fig. 4.)

Jan. 10th. 2^h to $3^h\ 0^m$. The planet was a beautiful telescopic object; its silvery lustre was however somewhat dimmed in places by faint and ill-defined markings. An irregularity in the terminator, near the S. limb, was steadily seen. (*a*. Fig. 5.)

* These drawings were exhibited at the meeting, and can be seen at the Society's rooms.

Jan. 17*th.* $2^h\ 45^m$ to $3^h\ 10^m$. One faint marking was observed near the limb; and the terminator was evidently polygonal. (Fig. 6.)

Jan. 22*nd.* $2^h\ 30^m$ to $3^h\ 0^m$. A very delicate marking was noted near the S. limb of the planet; the terminator, as on the 17th, was extremely irregular in shape—two elevations were seen at *a* and *b* (fig. 7) the junction of the S. portion of the terminator with the limb was rounded off.

Jan. 27*th.* $2^h\ 45^m$ to $3^h\ 10^m$. Two faint and ill-defined markings were visible; shape of the terminator irregular. (Fig. 8.)

Feb. 11*th.* $7^h\ 10^m$ to $8^h\ 0^m$. The shape of the terminator was very remarkable. Although the planet was undoubtedly slightly gibbous, two cusp-like projections were noticed, that on the S. being rounded off. (Fig. 9.) (*Venus* appeared to be perfectly dichotomized on the evening of Feb. 13th.)

Feb. 20*th.* The planet was seen to great advantage about 3^h, but no definite markings could be detected, although it was evident enough that the disk was not uniformly bright. An oval white spot was remarked close to the dusky shade of the terminator. (*a*. Fig. 10.) The S. cusp was sharper and longer than the N. cusp, which was evidently truncated; the shading of the terminator in the neighbourhood of the S. cusp was also noteworthy.

On the same day at $6^h\ 30^m$, the planet was splendidly defined and two markings were visible—a long dusky streak, concentric with the limb, and an isolated dark spot—nearly central. The cusps were both sharp, and the intervening terminator convex. (Fig. 11.)

Feb. 23*rd.* $5^h\ 35^m$. A distinct but very delicate marking was observed. The N. cusp was truncated. (Fig. 12.)

Feb. 27*th.* 3^h to 4^h. Although the definition and atmosphere were favourable for observation, no traces of markings could be seen. The N. cusp was truncated. (Fig. 13.)

On the same day at 7^h a well-defined marking of a very irregular shape was noted. Both the cusps appeared to be sharp, but S. cusp projected most. (Fig. 14.)

Feb. 28*th.* $6^h\ 45^m$. A marking was observed not far from the planet's limb, it was very similar in form to that seen on the previous evening. Close to the terminator three small white spots were remarked. Both the cusps were sharp, but the S. cusp appeared to be prolonged beyond a semi-circle. (Fig. 15.)

March 2*nd.* 3^h. A faint L-shaped marking was seen. Outline of the terminator very irregular. Both cusps sharp. (Fig. 16.)

March 7*th.* $6^h\ 30^m$. Two faint streaks were observed at a short distance from the limb. (Fig. 17.)

March 13*th.* $6^h\ 15^m$ to $7^h\ 0^m$. A well-defined marking was visible, which extended from the S. limb to the centre of the illuminated portion of the planet, where it bifurcated and turned abruptly towards the terminator. No peculiarity was noticed in the appearance of the cusps, but the shape of the terminator was remarkably irregular. (Fig. 18.)

On *March 20th* at $7^h\ 30^m$, and on *March 22nd* at $6^h\ 40^m$, faint markings were perceived. (Figs. 19 and 20.) On the latter date the phosphorescence of the dark side was very clearly seen.

March 26th. $6^h\ 10^m$. The planet appeared to be covered with dusky spots, but they were very faint and badly-defined. A bright marking was noticed at *a* (Fig. 21), the N. cusp was sharper than the S. cusp, and projected farther.

On *March 28th* at 6^h, and on *March 29th* at $7^h\ 25^m$, the S. cusp was noted as differing greatly in shape from the N. cusp, but both were perfectly sharp. A faint white streak was seen on both days extending from the S. limb parallel to the terminator. (Figs. 22 and 23.)

April 1st. $6^h\ 30^m$ to $7^h\ 0^m$. A very distinct dark streak was observed nearly concentric with the limb, and not far from it; this marking was seen without difficulty with an achromatic by Peter Dollond of $2\frac{1}{8}$ aperture. (Fig. 24.)

April 2nd. $5^h\ 0^m$ to $5^h\ 30^m$. Planet beautifully defined. A faint marking was visible very similar in shape to the dark streak observed on the previous evening. Both the cusps were drawn out to very fine thread-like points. (Fig. 25.)

In making the above observations, I generally used the full aperture of my 4-inch achromatic; but I sometimes found that the details visible on the planet (even on the most favourable occasions) were brought out better with the aperture reduced to $3\frac{1}{2}$ or 3 inches.

I used various negative eye-pieces, from 90 to 210.

I may mention that a few days ago I examined some drawings made by a young friend of mine, Mr. P. Wyatt of Bedford, who diligently scrutinized the planet with an achromatic of $2\frac{3}{4}$-inch aperture, the general correspondence of our sketches on those occasions when we happened to observe the planet at the same time was very satisfactory.

Phenomena of Jupiter's Satellites. By the Rev. S. J. Perry.

The regular observation of *Jupiter's* satellites was commenced at this Observatory with a view of aiding in procuring as complete a record as possible of these phenomena. The observations have all been taken with the full aperture of the 8-inch achromatic of Troughton and Simms, a power of about 300 being generally employed. The time was observed with a Frodsham chronometer, which has been almost invariably compared, on each night of observation, with the sidereal clock immediately after transits have been taken, the change of rate of both chronometer and sidereal clock being thus almost entirely eliminated.

Date. 1873.	Satellite.	Phenomenon.	Observed G.M.T. h m s	G.M.T. from N.A.—Obs. m s	Observers.	Remarks.
Jan. 25	I.	Ec. D.	2 2 47·7 A.M.	−0 24·0	W. C.	Hazy, approx.
	II.	Ec. D.	3 33 42·0	−0 40·2	,,	Unsteady.
	I.	Oc. R. first seen	4 48 35·6	+1 24·4	,,	,,
	II.	Oc. R. first seen	7 25 2·5	+0 57·5	,,	Very unsteady.
27	I.	Tr. E. int. contact	8 25 13·8 P.M.	+5 4·9	S. P.	,,
		ext. contact	26 36·3			
	IV.	Oc. R. first seen	9 3 15·9	+1 44·1	,,	
28	II.	Oc. R. first seen	8 33 57·5	+0 2·5	,,	Very unsteady.
Feb. 4	IV.	Tr. I. ext. contact	9 40 15·4		W. C.	Satellite almost as dark as shadow, darker than any bands.
		bisection	47 8·9	+8 51·1		
7	I.	Ec. D.	5 49 17·2 A.M.	+0 16·1	,,	
20	II.	Tr. E. bisection	9 33 6·5 P.M.	+2 53·5	,,	Hazy.
		ext. contact	38 10·0			
Mar. 3	III.	Sh. I. bisection	9 28 45·1	−3 45·1	S. P.	Glimpses through clouds, definition very good.
		Tr. E. bisection	11 25 41·0	+2 19·0	,,	Very cloudy.
		ext. contact	28 7·5			
5	I.	Tr. I. ext. contact	9 16 34·0		,,	Unsteady.
		bisection	17 59·9	+0 0·1		
		Sh. I. bisection	9 46 55·0	−1 55·0	,,	Bright ring.
		int. contact	48 16·6			Round shadow.
		Tr. E. int. contact	11 32 55·9		,,	
		bisection	34 39·4	+3 20·6		
		ext. contact	36 14·9			
		Sh. E. int. contact	11 59 56·0		,,	Misty.
		bisection	1 1·0 A.M.	+3 59·0		
8	II.	Oc. D. ext. contact	6 4 38·3 P.M.		W. C.	Cloudy.
		bisection	7 10·7	+1 49·3		
		Ec. R.	10 3 50·9	−0 57·1	,,	Full brightness 3m later.
11	I.	Ec. R.	4 37 34·9 A.M.	+0 7·5	,,	Full brightness 2m later.
12		Tr. I. ext. contact	10 58 48·5 P.M.			
		bisection	11 1 56·0	+1 4·0	,,	
		int. contact	4 46·5			
		Sh. I. first seen	11 40 11·0	−1 36·3	,,	
		int. contact	43 1·5			
13		Tr. E. int. contact	1 17 23·6 A.M.		,,	
		bisection	20 39·0	+2 21·0		
		ext. contact	24 45·4			
		Sh. E. last seen	1 57 37·2	+2 22·8	,,	Clouds.

Date. 1873.	Satellite.	Phenomenon.	Observed G.M.T.	G.M.T. from N.A.—Obs.	Observers.	Remarks.
			m s	h m s		
Mar. 13		Ec. R.	11 5 55·8 P.M.	+0 20·7	W. C.	Full brightness 3^m later.
14	II.	Tr. I. ext. contact	1 22 55·4 A.M.		,,	
		bisection	26 54·9	+0 5·1		
		int. contact	30 31·2			
		Sh. I. first seen	2 44 52·0	+3 24·3	,,	
		int. contact	49 56·5			
		Tr. E. int. contact	4 16 2·1		,,	Clouds.
		bisection	19 32·6	+0 27·4		Doubtful.
	I.	Tr. E. int. contact	7 46 18·0 P.M.		,,	Probably somewhat late.
		bisection	48 7·0	+1 53·0		
		ext. contact	51 18·5			
20		Oc. D. ext. contact	9 55 42·4		S. P.	
		bisection	56 54·9	+0 5·1		
21		Ec. R.	1 0 22·5 A.M.	+0 21·1	W. C.	Full brightness 4^m later.
22		Ec. R.	7 30 10·0 P.M.	−0 48·0	,,	Clouds, not more than 10^s late.
26	IV.	Tr. E. bisection	10 3 48·8	+3 11·2	S. P.	Very dark brown centre, with light brown ring; centre darker than any band. Very bright when near edge of disk.
27		Sh. I. first seen	1 30 12·6 A.M.	−0 53·6	W. C.	Tremulous.
		int. contact	34 0·1			
	I.	Tr. I. ext. contact	2 31 49·2		,,	Clouds, definition bad.
		bisection	35 21·7	+0 38·3		
		int. contact	38 8·7			
		Sh. I. first seen	3 29 49·2	−0 49·2	,,	Unsteady.
		Oc. D. ext. contact	11 38 15·5 P.M.		,,	
		bisection	41 16·4	+2 43·6		
28	I.	Ec. R.	2 54 34·5 A.M.	+0 43·3	W. C.	Full brilliancy 5^m later.
	III.	Oc. D. ext. contact	7 44 43·3 P.M.			
		bisection	47 35·3	−1 35·3	S. P.	
	I.	Sh. I. bisection	10 0 28·4	−2 28·4	,,	
		Tr. E. int. contact	11 19 45·7		,,	
		bisection	21 47·4	+1 12·6		
		ext. contact	23 43·9			
	III.	Oc. R. bisection	11 26 28·9	+1 31·1	,,	
		Ec. D.	11 33 22·4	−1 20·8	,,	
29	I.	Sh. E. int. contact	0 13 2·0 A.M.		,,	

Date. 1873.	Satellite.	Phenomenon.	Observed G.M.T.		G.M.T. from N.A.—Obs.		Observers.	Remarks.
			m s		h m s			
		bisection	0 14 16·5		+3 43·5			Very indistinct.
Mar. 29	III.	Ec. R.	3 3 57·3		−0 39·5		W. C.	Unsteady.
	I.	Ec. R.	9 24 1·8	P.M.	−0 3·6		,,	Full brilliancy 2m or 3m later.
31	II.	Sh. I. bisection	9 15 6·4		−3 6·4		S. P.	
		int. contact	16 7·5					
		Tr. E. int. contact	10 5 2·0					
		bisection	7 43·0		+1 17·0			
		ext. contact	10 14·0					
		Sh. E. int. contact	11 57 34·6				,,	
		bisection	59 50·6		+5 9·4			
April 4	I.	Oc. D. ext. contact	1 26 56·0	A.M.			W. C.	Cloudy, tremulous.
		bisection	30 8·5		+1 51·5			
	IV.	Oc. D. ext. contact	2 20 23·7				,,	Clouds. Not good.
		last seen	23 22·2		+10 7·0			
	I.	Tr. I. ext. contact	10 52 31·5	P.M.	−2 37·0		S. P.	Clouds.
		int. contact	56 42·5					Probably rather late.
	III.	Oc. D. ext. contact	11 16 41·0	P.M.			,,	
		bisection	19 27·5		−0 27·5			
	I.	Sh. I. bisection	11 55 17·2		−2 17·1		,,	
		int. contact	57 5·7					Only approx.
5		Tr. E. int. contact	1 8 50·0	A.M.			,,	Clouds.
		bisection	10 6·0		+1 54·0			
		ext. contact	11 48·5					
		Oc. D. bisection	8 0 16·1	P.M.	−0 16·1		W. C.	Thin clouds.
7	II.	Tr. I. bisection	9 43 53·2		−3 53·2		S. P.	
		int. contact	45 59·2					Clouds, indistinct.
		Sh. I. bisection	11 49 57·0		−1 57·0		,,	Cloudy.
		int. contact	52 38·5					
8		Tr. E. int. contact	0 30 0·5	A.M.			,,	
		bisection	32 49·0		+0 11·0			
		ext. contact	35 46·5					
		Sh. E. bisection	2 35 45·9		+5 14·1		W. C.	Faint, unsteady.
	III.	Sh. E int. contact	8 47 45·1	P.M.			,,	Shadow very dark.
		bisection	51 5·6		+10 54·4			
12	I.	Sh. I. bisection	1 50 28·5	A.M.	−1 33·5		,,	Glimpses through clouds.
		int. contact	52 14·0					
13		Tr. E. bisection	9 28 42·8	P.M.	+0 17·2		S. P.	Mist, steady.
		ext. contact	30 50·3					
		Sh. E. int. contact	10 30 57·0				,,	Clouds, difficult.
		bisection	33 2·0		+3 58·0			
18		Tr. I. ext. contact	2 24 35·7	A.M			W. C.	Low, unsteady.

Date. 1873.	Satellite.	Phenomenon.	Observed G.M.T. h m s	G.M.T. from N.A.—Obs. m s	Observers.	Remark.
Apr. 18		bisection	2 27 34·7	+4 25·3		
		int. contact	29 57·2			
19	I.	Oc. D. ext. contact	11 37 46·0 P.M.		S. P.	Mist.
		bisection	39 45·5	+0 14·5		
20		Tr. I. ext. contact	8 59 26·5		,,	
		bisection	9 2 3·0	−2 3·0		
		int. contact	4 13·0			
		Sh. I. bisection	10 13 22·6	−1 22·6	,,	
		int. contact	14 45·6			
		Tr. E. int. contact	11 17 14·1		,,	Passing mist.
		bisection	19 28·1	+0 31·9		
		ext. contact	21 19·1			
	IV.	Oc. R. bisection	11 40 28·1	−0 28·1	,,	
		ext. contact	43 57·6			
21	I.	Ec. R.	9 37 3·7	+0 1·2	,,	Full brightness 2^m or 3^m later.
23	III.	Tr. E. int. contact	0 1 30·0 A.M.		W. C.	Tremulous.
		bisection	6 16·5	+2 43·5		
		ext. contact	10 43·5			
	II.	Oc. D. ext. contact	9 40 29·3 P.M.		S. P.	
		bisection	43 28·3	+0 28·3		
29	I.	Sh. E. bisection	8 54 9·5	+0 50·5	W. C.	Faint, unsteady.

Stonyhurst Observatory,
May 2, 1873.

Observations of Procyon as a Double Star.

By O. Struve.* (Abstract.)

(*Communicated by the Astronomer Royal.*)

For the last twenty-two years the author has made one or two comparisons every year of this star with two telescopic stars, about six minutes of R.A. on each side of it, with the view of obtaining material for a confirmation of Bessel's theory of its irregular proper motion. On the 19th of March last whilst thus observing it, under exceptionally favourable atmospheric

* Communicated to the Academy of Sciences of St. Petersburg, 1873, April 8. Abstracted by W. T. Lynn, B.A.

conditions, he detected a faint point of light which followed *Procyon* at a very small distance, nearly on the same parallel. After ascertaining that this object was visible in the same manner in all parts of the field, and with eye-pieces of different power, he compared it micrometrically with the principal star. Three determinations of the position-angle, three measures of distance, and finally three more determinations of position-angle gave, with excellent agreement, distance = 11″·68, position-angle = 86°·8. The brightness of the small point of light (henceforth to be considered a companion of *Procyon*) was estimated at about two classes of magnitude less than the companion of *Sirius* discovered by Alvan Clark, of which the author had succeeded in obtaining a good measure only a few minutes before, *Sirius* being scarcely 13° above the horizon ; a sufficient proof that the *Procyon* companion was not a false image produced by impurity of the object-glass or other defect. As a still more complete test of this, the author attempted to make another observation in the second position of the instrument, but unfortunately whilst reversing it, the sky became covered with a thick stratum of cloud, and nothing more could be done that evening.

Having so frequently observed *Procyon* on previous occasions, with the special object of searching for a satellite, without any result, the conjecture at once presented itself that the object in question was a satellite, whose orbital motion had only recently brought it so far out of the rays of the bright star as to be perceptible. It could not possibly be a small star only optically connected with *Procyon*, because if so it would have been 24″ from *Procyon*, and therefore much more easily visible in 1851, when these measures were commenced, and the neighbourhood of *Procyon* carefully examined.

A reference next morning to Auwers' paper of 1862* appeared to support the probability that the object now discovered is identical with the hypothetical body supposed in that paper to be disturbing the motion of *Procyon*.

Even on the night of the discovery, any idea that the point of light was the effect of any ocular illusion, was disproved by the fact that it was also seen by the author's assistant, Herr Lindemann ; and any doubt that it might be due to any peculiar atmospheric reflection was dispelled by subsequent observation. The whole of these are contained in the following table, in which the position-angles are each the mean of three determinations, and the number of measures of distance are given in parentheses after each measure. The power III. (magnification, 309 times) was used throughout, excepting on March 29th for the second position, when the next higher power (412 times) was employed.

* An abstract of this paper will be found in *Monthly Notices*, vol. xxiii. pp. 18–20.

Date. 1873.	Sidereal Time. h m	Distance.	Sidereal Time. h m	Position Angle.	Remarks.
Mar 19	7 50	11·68 (3)	7 42	86·8	Images excellent. All measures in first position.
				86·9	
23			9 13	88·3	First position.
			9 41	87·6	Second position. Images not satisfactory. Companion only momentarily seen. Measures of distance impossible.
26	Images generally tolerable, but companion not seen with certainty.				
28			8 45	85·3	Observed in Position II. Light mist and companion only momentarily visible.
	9 0	12·09 (1)	9 8	89·0	After return to Position I. images better. Distance-measures very difficult. Herr Lindemann also saw companion in both positions.
	9 20	12·28 (1)	9 31	88·6	
29	8 5	12·37 (3)	7 55	85·8	Position I. Images beautiful. Companion seen easily; also by Herr Wagner.
			8 28	85·9	
	8 55	12·71 (1)	8 48	90·1	No more possible, images becoming tremulous.
30			8 14	87·8	Position II. Too tremulous afterwards to see companion.
31	8 25	14·07 (1)?	8 7	90·2	Position II. In dark field the companion always visible, but not sufficiently steady for satisfactory observation. The first measures of distance bad.
	8 30	12·76 (1)	8 45	89·0	
	8 55	13·04 (1)			
April 2			8 14	88·4	Position I. Images good.
	8 45	14·62 (1)	8 35	90·5	Position II. In first set, images tremulous from light mist. Afterwards sky clear, but eye fatigued. Companion visible the whole evening, but measures very difficult.
	9 6	11·97 (2)	9 22	85·8	

In conclusion, the author hopes to see his observations of this small companion-star of *Procyon* confirmed by other observers with other instruments. On the night of the discovery (March 19th) he wrote to Mr. Newall at Gateshead, requesting him to look for it with Cooke's 25-inch object-glass. Unfortunately, however, that gentleman was absent at the time, and before his return it is is to be feared that daylight will stand in the way of observations

in that part of the sky. We must probably therefore wait for next winter for a complete confirmation of the discovery of this interesting object. As the period determined by Auwers for *Procyon* in the paper above referred to, amounts to forty years, it is not likely that when observations can be resumed, the companion will be again so near the principal star as to be lost in its rays.

New Nebulæ discovered at the Observatory of Marseilles.
By M. E Stéphan.

M. Stéphan announces his having detected about 300 new nebulæ, but that the positions of 75 only have been accurately determined by comparision with catalogued stars. The following list of fifteen is in continuation of previous ones. The positions of the remainder will follow as they are ascertained.

Positions moyennes pour 1872·0.

Star de Comp.	R.A. h m s	P. ° ′ ″	
a	17 8 43·47	49 35 12·7	e. e. F.—13 precède de $0^s·4$ (à peine observable.
b	19 13 1·20	44 13 29·5	e. e. F.—(à peine observable.)
c	20 29 39·26	95 3 43·6	e. e. F.—m. E.—Vap.—i. R.—Leg. cond. au C.
d	21 15 39·42	51 22 12·1	e. P.—e. F.—R.—ext. petite, se projette sur la néb. près du centre.
e	21 35 8·32	94 17 28·6	e. e. P.—e. e. F.—R.—Cond. au C.
f	21 38 13·63	94 11 26·4	e. P.—e. F.—R.—Cond. au C. mais pas de P. B.
g	21 56 51·38	79 24 33·9	t. P.—t. F.—R.—g. B. M.
h	22 6 1·66	51 54 33·7	e. P.—e. F.—R.—Leg. Cond. au C.
h	22 6 18·68	51 55 57·3	e. P.—e. F.—R.—Leg. Cond. au C. (un peu moins faible que la précédente.)
i	22 9 58·40	71 24 17·1	t. F.—I.—E du S au N (1′ sur 0″·3) —* 10 en contact au S.
k	22 10 15·95	74 9 57·9	e. e. P.—e. e. F.—R.—Cond. au C.
l	22 22 32·07	60 21 41·8	P.—e. e. F.—Ov.—Leg. Cond. au C. —p. * projetée près du bord.
m	22 29 41·06	55 51 16·1	e. e. P.—e. F.—R.—Cond. au C.
n	22 32 24·05	55 9 55·7	e. P.—e. F. (à peine observable)—Vap.—Leg. Cond. au C.
n	22 32 55·30	55 7 27·2	e. e. P.—e. e. F.—I.—all.—Cond. excentrique.

Abbreviations.

e. P.	excessivement petite.
e. e. P.	excessivement, excessivement petite.
e. F.	excessivement faible.
e. e. F.	excessivement, excessivement faible.
E.	étendue.
m. E.	modèrement étendue.
t. P.	très petite.
t. F.	très faible.
R.	Ronde.
i. R.	Irrégulièrement ronde.
I.	Irrégulière.
g. B. M.	Graduellement brillante des bords au centre.
Vap.	Aspect vaporeux.
P. B.	point brillant.
Cond. au C.	Condensation au centre.
Ov.	Ovoide.
All.	Allongée.

Etoiles de Comparaison. Positions moyennes adoptées pour 1872·0.

		R.A.	P.
		h m s	° ′ ″
a	93 W. H. xvii. (8·9) N. C.	17 4 34·74	49 40 9·4
b	2852 Arg. Z. + 45° (9·4)	19 8 35·00	44 11 25·8
c	697 W. H. xx. (9) A. C.	20 28 22·59	95 1 56·1
d	311 W. H. xxi. (9) N. C.	21 13 54·35	51 22 34·2
e	778 W. H. xxi. (9) A. C.	21 33 36·40	97 23 31·5
f	730 W. H. xxi. (8) A. C.	21 31 58·74	94 9 29·6
g	4677 Arg. Z. + 10° (9·0)	21 55 48·20	79 28 8·3
h	4697 Arg. Z. + 38° (9·3)	22 4 46·80	51 53 7·5
i	43557 Lalande (8)	22 12 54·35	71 21 9·4
k	218 W. H. xxii. (7·8) A. C.	22 11 53·49	74 14 57·0
l	354 W. H. xxii. (8) N. C.	22 16 41·26	60 17 16·6
m	4721 Arg. Z. + 34° (9·5)	22 29 3·90	55 48 28·0
n	791 W. H. xxii. (9) N. C.	22 34 27·21	55 6 31·1

A Self-recording Transit Micrometer.

By the Rev. S. J. Perry.

At the November meeting of the Society in 1871, a paper was read by Professor A. Herschel on a self-recording transit micrometer. The principle was ingenious and simple, but the difficulties in practice appeared great on account of the arrangement of the springs. The nice adjustment of strength, that must

be first obtained, and then preserved, so that the first spring may be completely compressed before the rest are at all disturbed, and then the second be similarly compressed, and so on, appears scarcely capable of attainment. I venture, therefore, to propose a simpler method of arriving at the same desirable result. A few words, with the annexed figure, will suffice to explain the idea, which has been suggested to me by Mr. J. Hostage, the first assistant at this observatory.

An excentric disk A, Fig. 1, moving with the micrometer screw, lifts a rod B, whose upper surface is tipped with platinum, and which, whilst rising, gently compresses the spiral spring, E, placed within an insulating guide. When B reaches C, whose lower surface is of platinum, and which rests on a platinum point D, the contact completes the circuit, which is instantaneously broken again by the removal of C from D, from the continued upward motion of B. As B is forced down from its highest point by the spiral spring a second circuit is made and broken as C reaches D. Two observations will thus be registered on the chronograph at each revolution of the screw, and they can obviously be arranged, so that the intervals shall be all equal.

If a single observation at each revolution of the screw is preferred, the following arrangement may be adopted to prevent the second contact.

Instead of the straight spring, that keeps C in position, a weight H, Fig. 2, is made to balance C in such a manner that almost as soon as B has raised C above D, H will remove C from contact with B. The projecting piece G, attached to B, will force C down again upon D as B approaches its lowest point.

The pitch of the screw will regulate the number of transit observations required without any additional contrivance, and the intervals, corresponding to a whole or half revolution of the screw, will all be equal.

The micrometer will work equally well in both directions.

The upper part of C, and the whole of F, must be made of an insulating substance. A wire through F connects D with one of the poles of the battery, the other pole being in direct connexion with the micrometer frame. The battery connexions are the same for any number of transit wires, and can easily be kept in order.

The whole self-recording apparatus being external to the micrometer can readily be applied to an existing apparatus.

Stonyhurst Observatory,
February 12th, 1873.

Addendum.—The utility of the preceding arrangement for a self-recording micrometer will be made more apparent by showing that it is applicable to other observations besides those of transit.

Fig. 3 represents a modification of the ordinary parallel wire micrometer, adapted for the permanent record of the angular

distances between any bodies that can be brought within the same field of the micrometer, and giving at the same time the exact instant at which such measures were taken.

The micrometer screws differ from those of an ordinary instrument in having a wider pitch of thread near the screw heads than towards the centre of the micrometer, so that the former may give a greater horizontal movement to the arms I and I′ than that of the wires in the field of view.

A light brass cylinder K, on which metallic paper may be secured by clamps, rests on bearings attached to the base plate of the micrometer, the lower part of which is slotted so as to guide the arms I and I′. This cylinder moves with the micrometer screw α, being kept in motion, whilst the measurements are being taken, by a small toothed wheel m attached to the spindle of the screw, and gearing into the large wheel L on the axis of the recording cylinder.

The instrument is adapted for the observation of the passage of a spot, or of an inferior planet, across the solar disk, or for measuring the near approach of the Moon to celestial bodies, and it might be of use in the transit of the shadows of *Jupiter's* satellites, which are better seen when well on the disk than at first or last contact. Distances between double stars, diameters of planets, and similar observations, might also be recorded in this way, but then the chronograph connexion would be unnecessary, and the space traversed by either wire being small, a very enlarged movement might be given to the recording arms.

The working of the instrument will be easiest understood by considering its application in some particular case, such as the micrometric measurements of *Venus* in transit. The wire moved by the screw that actuates I must be kept always tangent to the Sun's limb, the brass point at the extremity of I being in continuous contact with the metallic paper. As the cylinder K rotates the brass point of I will describe a line more or less straight according to the steadiness of the driving power of the equatorial clock.

The second wire of the micrometer, which moves with I′, is made to follow the preceding or following limb of *Venus*. As this screw rotates it imparts a slow motion to the recording cylinder, and at a fixed reading of the micrometer it also completes the galvanic circuit by raising B to C, as explained for the transit micrometer.

A small electro-magnet, introduced in the circuit at the lower extremity of the arm I′, brings the brass point or sharp pricker of I′ into connexion with the recording cylinder at the same instant that the time-singal is registered on the chonograph. The electro-magnet may be conveniently brought down on to a bar of soft iron P, which will serve at the same time to strengthen the frame. A covering of thin brass for P will prevent adhesion when the circuit is broken. The wire S forms the connecting link between the electro-magnet, and the brass micrometer.

Fig. 4 is a side view of the revolving cylinder and driving wheel. T is a spring that allows the electro-magnet to bring down the pointer of I′ on the cylinder at *u*.

The great advantage of this instrument depends on there being no necessity of reading the micrometer during the observation, but the value of the scale on the cylinder can be tested by readings at other times.

Stonyhurst Observatory,
March 10th, 1873.

Search for Vulcan. By H. C. Russell, Esq.

(*From a Letter to the Honorary Secretaries.*)

I send you the enclosed note for the *Notices*, as it is probable many in reply to Mr. Hind's invitation will have been on the look out for the supposed planet.

Circumstances here were specially favourable on the 24th March, the sky being clear all day.

I used the $7\frac{1}{4}$-inch refractor, and at times an unsilvered $10\frac{3}{4}$-inch glass reflector, with both of which the very rapid changes going on in the large spot were distinctly seen during the day.

The mail closes this morning at 10 A.M., so I am obliged to send the results obtained to 9 A.M.

A careful watch was kept for the supposed planet all day of the 24th, but nothing could be seen at all like it. Regular observations were also made on the 21st, 22nd, and 23rd, with no better result.

This morning the sun rose obscured by the clouds, and was not observed until 9 A.M., when nothing except the sun-spots could be seen.

Sydney Observatory,
25th March, 1873.

A Second Catalogue of New Double Stars, discovered with a 6-inch Alvan Clark Refractor. By S. W. Burnham, Chicago, U.S.A.

Nearly all of these double stars have been found since the preparation of the former catalogue. The exceptions are where some uncertainty existed as to the real duplicity of the star, or where the distance and angle had not been sufficiently observed. The 6-inch Clark Refractor, previously referred to, has been used exclusively. From a careful examination of a General Catalogue of Double Stars (in manuscript), comprising substantially, if not entirely, every double star known, I have little doubt that the following are now noted for the first time as double stars. The numbers are continued from the first list. The position angles and distances are estimated in all cases.

No	Designation.	R.A. 1870. h m s	Decl. 1870. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
82	ω Andromedæ A and B C	1 19 53	44 44	105	138	5	Dec. 2, 1872	A very minute double companion to ω *Andromedæ*.
	B and C	..	..	..	2	12, 12		
83	Weisse ii. 666 = L. 5140	2 39 29	− 5 30	125	1	7, 10	Dec. 16, 1872	A beautiful but difficult pair in *Cetus*, $4^m\ 22^s$ p. No. 10 of my former list.
84	Weisse iii. 147	3 9 35	− 6 24	20	0·5	6, 6	Nov. 16, 1872	Visible to the naked eye. Excessively close and difficult, but the elongation seems certain.
85	Weisse iii. 1031	48 0	17 15	225	5	8½, 12	Feb. 14, 1873	In *Taurus n f* a brighter star.
86	Weisse iv. 129	4 8 3	23 11	60	2	9, 10	Feb. 16, 1873	Faint but pretty pair in *Taurus*.
87	B. A. C. 1342 = P. iv. 53	14 43	20 28	180	1·5	7, 11	Feb. 7, 1873	An extremely beautiful pair, *n f* ω^2 *Tauri*. The colours were fine and strongly contrasted. This star is No. 34 of Schjellerup's Catalogue of Red Stars. Variously rated in the Star Catalogues from 6th to 7th mag.
88	51 Eridani	31 3	− 2 44	90	25	6, 15	Dec. 8, 1871	Very minute companion.
89	Weisse v. 752	5 30 59	− 1 30	340	0·6	9, 9	Feb. 19, 1872	One of the most difficult double stars I have ever found. It follows ε *Orionis* $1^m\ 23^s$, and is 12′ 57″ *s*. This is given in Lalande as 7½ mag. (= L. 10608.)
90	26 Aurigæ A and C	30 16	30 25	115	30	5, 12	Mar. 19, 1872	A second companion, though not difficult, missed by Struve, and not mentioned by Dawes, Dembowski, and others, in measures of A and B.
91	Weisse v. 1293	39 34	20 53	80	1·5	8, 12	Feb. 16, 1873	A beautiful and delicate pair in the same field with O Σ 118, 43^s *p*. and 4′ *n* (= L. 10913.)
92	Weisse v. 1309	40 22	21 3	170	5	9, 11	Feb. 16, 1873	Near the preceding pair.
93	Weisse v. 1332 A and B C D	5 41 7	21 0	125	60	9	Feb. 16, 1873	A star with curious triple companion, the two smaller stars being very minute; 31^s *f* O Σ 118 and 9′ *n*. A very low power will include this and the two preceding pairs and O Σ 118 in the same field.
	B and C	..	..	300	3	11, 12		
	B and D	..	..	160	2	13		

No.	Designation.	R.A. 1870. h m s	Decl. 1870. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
94	L. 11086	5 43 40	−14 31	190	2·5	6, 10	Jan. 26, 1873	A fine and easy double star about 45′ from ζ *Leporis*, *n f*. Just visible to the naked eye.
95	L. 11128	45 39	− 7 20	300	10	8, 15	Feb. 25, 1873	Near 55 *Orionis*, *n f*. The companion very small, and requires the highest powers to be satisfactorily seen.
96	75 Orionis A and B	6 9 56	9 59	160	120	6, 9	Jan. 26, 1873	A companion to the companion, the smaller star being extremely faint.
	B and C	..	..	250	2·5	14		
97	L. 12260	18 6	− 1 21	260	1	7½, 10	Feb. 19, 1873	An exquisite pair in *Orion*, but very difficult; 2^m *p*, a 6½ m. star.
98	L. 12564	26 13	− 5 15	130	1	8, 8	Feb. 19, 1873	A difficult double star; in *Monoceros*.
99	Weisse vi. 1610 A and B	53 20	12 54	300	8	8, 14	Mar. 16, 1873	The *n* star of a wide pair (60″ ±). The close companion excessively minute.
	A and C	..	..	240	12	12		
100	Weisse vi. 1620	53 39	12 37	270	2·5	8, 12	Jan. 3, 1873	A very delicate object. This star and the preceding form with a third 8 m. star an equilateral triangle. The latter is also a wide double (= H. 3288.)
101	9 Argus	7 45 44	−13 33	120	0·7	5½, 5½	Mar. 11, 1873	A very close, and from its southern declination, difficult pair. The components very nearly equal.
102	L. 16234	8 10 30	− 8 37	110	3	7, 11	Mar. 11, 1873	Delicate pair in *Monoceros*.
103	L. 17611	8 48 32	− 7 19	80	2	8, 12	Feb. 7, 1873	Very unequal; 35^s *p* 17 *Hydræ* (Σ 1295), and 9′ *n*.
104	L. 18134	9 4 49	0 49	120	2·5	7, 12	Feb. 17, 1873	Another fine and difficult double star in *Hydra*.
105	ν Leonis	17 4	26 44	210	2	5, 11	Feb. 25, 1873	The companion an exceedingly minute but bright point of light, making a delicate and interesting object.
06	μ Libræ	14 42 12	−13 36	340	1·3	5, 7	Apr. 12, 1873	A fine and very easy pair. There must be relative motion, for it could not have escaped Struve and others if as wide formerly as now. A very small aperture is now sufficient to show it.

On Logarithmic Tables.

By J. W. L. Glaisher, B.A., Fellow of Trinity College, Cambridge.

In a paper entitled *Einige Bemerkungen zu Vega's Thesaurus Logarithmorum*, which was published in the *Astronomische Nachrichten*, No. 756, for May 2, 1851 (reprinted *Werke*, t. iii. pp. 257–264), Gauss has examined, at a considerable expenditure of care and trouble, the relative numbers and magnitudes of the last-figure errors that occur in the sine, cosine, and tangent columns of Vega's *Thesaurus* (Leipzig, 1794, fol.). The principle of the investigation is as follows : Gauss remarked that the tabular results in the sine column were almost without exception equal to the sum of the corresponding tabular results in the cosine and tangent columns. Now, if the tabular results in the three columns were all true to the nearest unit, this agreement would only happen on the average three times out of four, so that we may feel certain that one of the columns was obtained by simple addition or subtraction from the other two. It therefore became an object of importance to determine which of the three columns had been deduced (and was therefore liable to greater errors), and to effect this it was necessary to compare some of the (ten-decimal) tabular results in Vega that had elsewhere been calculated to a greater number of places with their more extended values. Gauss accordingly made four series of such comparisons, the scheme of one of which I here reproduce (the comparison being for the 21 angles between 15° 38′ 20″ and 15° 41′ 40″ by means of a fourteen-figure manuscript of Gauss's own calculation):—

	Sine.	Cosine.	Tangent.
0	4	12	1
1	9	8	8
2	6	1	6
3	2		4
4			2

The meaning of this is, that in the 21 angles 4 sines were found correct, 9 affected with an error of 1, 6 with an error of 2, and 2 with an error of 3 in the last figure; and similarly for the cosines and tangents. The scheme I have quoted is a little anomalous, as it gives rather more errors than the average of the whole table would show; but there are objections of a more serious character to the other schemes, that will be referred to further on.

The general tenor of the results of all the comparisons, is that the tangents are the most inaccurate, having much the greatest number of large errors, and that the cosines are more accurate than the sines. This leaves no doubt that the tabular results in the tangent column were obtained by subtracting the

corresponding tabular results in the cosine column from those in the sine column; and the fact that the cosines are more accurate than the sines, Gauss regards as due, at least in part, to the process of interpolation having been simpler for the former, although other causes may have operated. ("Dass die Zahlen der Cosinuscolumne weniger ungenau sind, als die der Sinuscolumne, rührt wohl ohne Zweifel wenigstens theilweise, daher, dass bei den erstern die zur Ausfüllung erforderlichen Interpolationsmethoden einfacher ausfallen, möglicherweise können indess noch andere Ursachen mitgewirkt haben, worüber sich nur unsichere Vermuthungen aufstellen lassen würden.")

The explanation which I now proceed to give of the greater accuracy of the cosine column is—there can be little doubt—the true one, as it rests on a statement of Vlacq, the calculator of the table, which Gauss must have overlooked. Before quoting the statement in question, however, it is desirable to explain more fully the origin of the trigonometrical canon that forms rather less than half of Vega's *Thesaurus*, and with which alone we are now concerned. This canon gives logarithmic sines, cosines, tangents, and cotangents for every second from 0° to 2°, and for every ten seconds from 2° to 45° to ten decimal places arranged in the usual semiquadrantal manner. The great bulk of the table was reprinted from Vlacq's *Trigonometria Artificialis* (Gouda, 1633), which gives logarithmic sines, cosines, tangents, and cotangents for every ten seconds of the quadrant; and the values of the functions for every second for the first two degrees were computed by Lieutenant Dorfmund of the Royal Artillery, with the assistance of other members of the corps, under Vega's own direction (*Thesaurus*, p. xxi). What follows must be understood as having reference only to the part of Vega that was reprinted by Vlacq, as I have made no attempt to determine the manner in which Lieutenant Dorfmund performed his portion of the work (amounting to only about a quarter of the whole); Gauss, though noticing the difference of origin of the two portions of the table, has not always kept them distinct in making his comparisons. Now, in the preface to the *Trigonometria Artificialis*, Vlacq himself gives an account of how the canon was constructed, his words being, "Primùm Logarithmos Sinuum à 45 Grad. usque ad finem Quadrantis qui habentur in Opere Palatino, investigavi ope Chiliadum Centum Logarithmorum de quibus supra dixi, juxta modum qui hic traditur pag. 4. quos adeò exactè ita acquisivi, ut in numeris tam magnis supra unitatem in postremâ eorum notâ nihil abundet sive deficiat. Ex illis deinde reliquos omnes Logarithmos Sinuum ab initio Quadrantis usque ad 45 Gradus per solam Additionem et Subductionem inveni juxta hanc Regulam; Duorum arcuum Quadrantem conficientium, si Logarithmus Sinus minoris duplicati addatur Logarithmo Sinus 30 Graduum; et à summâ subtrahatur Logarithmus Sinus majoris, reliquus erit Logarithmus Sinus minoris; Cujus rei demonstrationem videre licet in Trigonometriâ Britannicâ Cap. 16.

Atque sic acquisitis Logarithmis Sinuum, Logarithmos tangentium per solam Subductionem habere potui. . . . Atque ita totum Canonem absolvi."

The formula expressed analytically is

$$\log \sin 2x + \log \sin 30^\circ - \log \sin (90^\circ - x) = \log \sin x$$

so that the process was to calculate first all the log cosines by simply taking out the logarithms of the sines of the angles in the second half of the quadrant as they appear in the *Opus Palatinum*, which gives natural sines, tangents, and secants for every ten seconds of the quadrant to ten places. The log sines were then deduced by means of the formula

$$\log \sin x = \log \cos (90^\circ - 2x) - \log \cos x - \log 2$$

and then the log tangents from

$$\log \tan x = \log \sin x - \log \cos x$$

It thus appears that no interpolation at all (except such as is necessary to take out an ordinary ten-figure logarithm) was used in the construction of the canon; and the reason for the errors being such as they were found to be by Gauss is apparent. The log cosines are merely subject to the ordinary last-figure errors with which the taking out of ten-figure logarithms is always liable to be attended, and which, though frequently as large as $\pm$ 1, do not very often amount to so much as $\pm$ 2. The log sines, deduced from them by a formula, are much more liable to large errors, as we may expect a large error whenever the log cos $(90^\circ - 2x)$ and log cos x are affected with moderate errors of opposite sign; and, further, as log 2 = ·30102 99956 6398 . . . which would be used as ·30102 99957, we see that if log cos x is too great by an error α, and log cos $(90^\circ - 2x)$ too small by β, then the error of log sin x is $\alpha + \beta + \cdot 36$.

The log tangents are of course the worst off of all, as in effect they are calculated from the formula—

$$\log \tan x = \log \cos (90^\circ - 2x) - 2 \log \cos x - \log 2$$

so that under the circumstances just mentioned the error would be $2\alpha + \beta + \cdot 36$. Vlacq himself acknowledges his cosines may be erroneous to the extent of a unit (and the error may even amount to 2 units), so that we see that an error of 2 would not be a very unusual occurrence in the sine, nor would one of 3 be so in the tangent, while greater errors still would occasionally arise.

At the end of his *Arithmetica Logarithmica* of 1628, Vlacq gave a ten-figure trigonometrical canon to every minute, the mode of construction of which he explains in the preface to that work in nearly the same words as those above quoted from the preface to the *Trigonometria Artificialis*, with the difference that the original natural cosines were taken from Rheticus's *Thesaurus*

Mathematicus (instead of the *Opus Palatinum*), and that to the statement that the log cosines never differ from the truth by more than a unit, is added the ground Vlacq had for the assertion, "quod ex differentijs differentiarum, quæ ferè æqualiter decrescunt, explorare potui."

The *Thesaurus Mathematicus* contains Rheticus's great canon of sines for every ten seconds to fifteen places, and was published at Frankfort after his death by Pitiscus, in 1613 (misprinted 1513 in the first two title-pages); while the *Opus Palatinum*, which contains, as before remarked, a complete ten-place canon of all the functions for every ten seconds, was calculated by Rheticus, and published after his death, in 1596, at Neustadt, under the editorship of Valentine Otho. A portion of the latter canon, which was found to be partially erroneous, was corrected by Pitiscus, in later copies, on which see De Morgan, Article 'Tables,' *English Cyclopædia*. It thus appears that either the *Thesaurus* or the *Opus Palatinum* would have answered Vlacq's purpose equally well, as (although I have made no comparison to make certain that such is the case) in all probability the last figures of the latter are true to the nearest unit. At all events, Vlacq knew both works, and we may take it for granted that the natural sines which formed the foundation of his table were all but free from error.

I can offer no satisfactory explanation of the fact that Gauss failed to notice Vlacq's own description of the manner in which the canon was constructed. It is possible that he had not a copy of the *Trigonometria Artificialis* at hand to refer to, or that in the copy he did refer to the preface was wanting, as it is hardly conceivable that it should not have occurred to him to see if Vlacq himself had given any information on the matter.

Reverting to Gauss's paper, besides the comparison, the scheme of the result of which was quoted near the commencement of this communication, there are two others which refer to the portion of the table calculated by Vlacq; these are:—

	Sine.	Cosine.	Tangent.
0	51	65	46
1	49	35	43
2	..		10
3			1

the result* of a comparison for the 100 angles that are multiples of 27′; and formed from the errata-list at the end of Hobert and Ideler's Tables, and

	Sine.	Cosine.	Tangent.
0	29	56	36
1	52	25	42
2			3

* Gauss has included in his scheme multiples of 5′ 24″ also, but I have omitted those that occur only in the tabular results calculated by Dorfmund.

which results from a comparison for every third minute between 14° 0′ and 18° 0′ inclusive, the number of angles being thus 81. This comparison was made by Gauss, with Briggs' *Trigonometria Britannica* (Gouda, 1633), which (besides natural functions) gives log sines and tangents for every *hundredth of a degree* of the quadrant to fourteen places (semi-quadrantally arranged). It thus appears that every third minute is an argument common to both the *Artificialis* and the *Britannica*.

Now a mere glance at the three schemes written above shows that how much more inaccurate are the sines and cosines in the first than in the other two, where they appear as quite free from errors greater than unity. This, Gauss himself remarked, and he pointed out that errors greater than unity were not unusual for arguments that do not occur in the *Britannica*. This, at first sight, might seem to imply that Vlacq had made use of the *Britannica* (which he printed at his own expense after Briggs's death) to correct his own canon by. It is a *primâ facie* argument against this view, that if Vlacq had corrected any of his values, he would have corrected them thoroughly, and not consistently left a unit error remaining; but there is no occasion to appeal to probable reasoning at all, as the perfect independence of Vlacq's and Briggs's calculation is placed beyond all doubt by the fact that all the tabular results dependent on arguments common to both the *Artificialis* and the *Britannica* had appeared previously in Vlacq's *Arithmetica* of 1628. This is evident, if it is remembered that the latter work contains a *minute* canon, and that the arguments common to the two former works are all multiples of 3′. The tabular results in the *Arithmetica* were reprinted without alteration (at all events generally) in the *Artificialis*, as I have found by comparing them for every third minute between 14° and 18° (the angles included in the last-written scheme), and by miscellaneous comparisons of different pages of the two works chosen at random.

The difficulty of seeing any explanation that would account for the greater deviations from accuracy in the case where the arguments are not included in the *Britannica*, induced me to form independent comparisons between the *Britannica* and the *Artificialis*, and also between the *Britannica* and Vega's *Thesaurus*, for the same arguments as those to which Gauss's schemes apply. The results were as follows:—

A comparison of the tabular results for the 100 angles that are multiples of 27′ between the *Britannica* and *Artificialis* gave the following scheme:—

	Sine.	Cosine.	Tangent.
0	43	64	41
1	49	36	41
2	7		15
3	1		3

and the comparison between the *Britannica* and Vega's *Thesaurus*, for the same arguments, gave

	Sine.	Cosine.	Tangent.
0	51	64	46
1	49	36	43
2			10
3			1

The results of a comparison between the *Britannica* and *Artificialis*, for the tabular results corresponding to every third minute between 14° and 18° (81 angles), are

	Sine.	Cosine.	Tangent.
0	25	57	34
1	52	24	41
2	4		6

while the comparison between the *Britannica* and Vega's *Thesaurus*, for the same arguments, gave

	Sine.	Cosine.	Tangent.
0	29	57	36
1	52	24	41
2			4

The first of these four schemes was verified also by comparing the *Artificialis* with the table in Callet for every thousandth of a degree (which was, however, no doubt formed from the *Britannica*). The second agrees with the results found by Hobert and Ideler, and quoted by Gauss, except that the number of errors in the cosine-column is given in the latter as 35 instead of 36. This arises from Hobert and Ideler not having included the error in 45°, thinking perhaps it was sufficient to notice its occurrence in sin 45°; the other two schemes call for no particular remark, except that it will be noticed that the last differs slightly from that given by Gauss, who seems to have made one or two trifling mistakes.

A glance at the two Vlacq schemes shows how exactly the distribution of the errors in the different columns is what we would expect from our knowledge of the manner in which they were calculated.

The nature of the corrections made by Vega is also rendered very apparent, viz. he corrected nearly all the errors amounting to as much as 2 in the sine-column,* the additional accuracy of the tangents being consequent thereon. How Vega found out these errors, I do not know: it could not have been by comparison with

* In the first of the two comparisons, I marked each tabular result with a 0, 1, 2, 3, according as it was correct or was in error, by one, two, or three units, and I find that every error amounting to 2 or 3 was corrected, but not a single one merely requiring an alteration of one unit was amended.

the *Britannica*, or he would have probably referred to it; but there is nothing in the *Thesaurus* to show that he even knew of its existence, much less made any use of it. Besides, considering Vega's love of accuracy (not to mention the fact of his having offered a reward for the detection of errors in his work), it is impossible to believe that had he used the *Britannica*, he would have been contented to leave unaltered so many unit-errors that he might have corrected, and only to remove those of greater amounts; while he certainly would not have allowed the errors of 2 in the tangent-columns to remain. Also, it will be observed that Gauss's comparison for angles not an exact number of minutes (see first scheme quoted in this paper), seems to show that such corrections have not been generally made in cases where the argument involves seconds as well as minutes. A theory that might explain this would be to suppose that Vega had corrected Vlacq's minute canon of 1628 by second differences; but I refrain from making conjectures, as a more detailed examination of the table than I have given to it could not fail to reveal the method that had been adopted. Such an examination I hope to be enabled to make, as it appears to be of very considerable practical importance to know the exact extent to which Vlacq and Vega (both of which, be it remembered, so far from being obsolete and only historically valuable, are unique of their kind, and still in daily use) are to be relied on. It is rather a scandal to the mathematical sciences that so little should have been done to improve upon the calculations of two centuries and a half ago. It is the knowledge of the extreme value of these fundamental ten-figure tables in mathematical calculations of all kinds, together with the conviction that the time must come when their republication will be a necessity, that appears to me to render the question of their accuracy one of the very highest importance; quite independently of the historic interest that every one must feel in the origin (or rather birth) of the quantities which are the means whereby every important numerical calculation is performed, and the instrument by which theoretical is transformed into practical science; and I may here remark that there is scarcely one of the principal mathematical tables that was not originally professedly calculated chiefly for its use in Astronomy (by facilitating the solution of spherical triangles).

I had the curiosity to make a comparison between the *Britannica* and *Artificialis* for arguments near the middle of the quadrant (where the sines and consines are nearly equal). The following shows the results for the 61 angles, which are multiples of 3′ between 43° and 45° :—

	Sine.	Cosine.	Tangent.
0	36	43	36
1	23	18	17
2	2		8

and it will be seen that the relative accuracy of the sine, cosine, and tangent columns remain the same in all essential respects.

Gauss, in his paper, remarked as one of the results of his examination, that for the angles 15° 40′ 20″ and 15° 41′ 30″ the errors for the log sine, cosine, and tangent are respectively 3, −1, +4 and +3, 0, and +4. I have examined these by substitution in the formula quoted above, from which Vlacq computed his log sines, and I find that it is rigorously satisfied. This would be a verification (if such were needed) that Vlacq did calculate his log sines in the manner he explains, and it further indicates that the log sine of 31° 20′ 40″, and that of 31° 23′, are inaccurate by at least a unit.

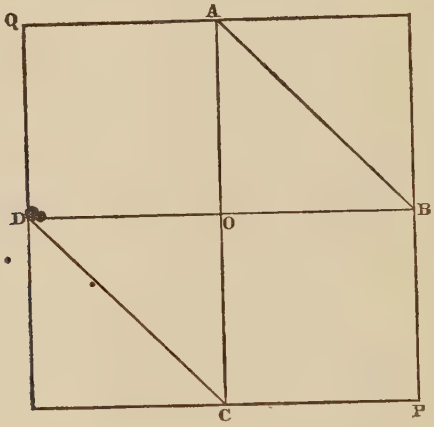
It appears, therefore, from the considerations developed in this paper that there is no mystery about the errors in Vlacq's *Trigonometria Artificialis*, their origin and magnitudes being accounted for in a perfectly satisfactory manner. But what is still left uncertain is the manner in which Vega examined and corrected the table; it seems that his method enabled him to remove large errors from the sine columns* (but not from the tangent columns), at all events for certain arguments that were an exact number of minutes, but not apparently for all the rest.

In the preface to his *Thesaurus* Vega offers a reward of a ducat for the first notice of any error which could give rise to a false calculation;—(German, "Der zu falschen Rechnungen Anlass geben kann;" Latin, "Pro singulis spalmatibus computationem turbantibus.") Whatever may have been the exact meaning Vega ascribed to these words (and, be it noted, he points out in the same preface errors of a unit in the last figure that he had detected in Vlacq), it is clear that had they been taken literally, he might have been awkwardly placed, as Gauss estimates the number of last-figure errors at from 31983 to 47746, and in fact, as the latter has remarked, been in much the same position as King Shiram. It will have been observed that Vlacq cannot fairly be charged with more than a few last-figure *errors*. He is responsible for the cases where in the log cosines the last figure deviates from the truth by more than a unit, and these may properly (with the accompanying deviations in the log sines and tangents) be laid to his charge as blunders made by him; but having stated that all his log cosines may be inaccurate to the extent of ± 1, and having explained his mode of calculation of the log sines and tangents, he throws upon the reader the responsibility of determining how far the latter are to be relied on.

The statement quoted from Gauss to the effect that an irrational quantity given to a fixed number of decimals, and obtained as the sum of two other irrational quantities similarly curtailed,

* Nearly all the errors which Vega mentions in his preface as having been corrected by him refer to arguments containing some seconds over; this would seem to imply that he set more value on the detection of such errors than those having reference to arguments consisting of an exact number of minutes.

will be erroneous on the average once out of every four times, amounts to the assertion that if x and y can each have any values between $\pm a$, and all these values are equally probable, then the chance that $x+y$ lies between $\pm a$ is $\frac{3}{4}$, or in other words that $\iint dx\,dy$ subject to the conditions $a>x>-a$, $a>y>-a$ and $a>x+y>-a$ is equal to $\frac{3}{4}a^2$. This can of course be proved by performing the integrations, but its truth is evident at sight by inspection of the subjoined figure, where A B P C D Q is obviously the area included within the limits of integration.



If $x-y$ were involved instead of $x+y$, A D and B C would be joined instead of A B and D C, and the result would be (as it clearly ought to be) the same as before.

Michael Taylor's trigonometrical canon to every second was calculated throughout to ten figures, and then contracted to seven, so that the results in this case ought to agree with the theory. I accordingly opened Shortrede (reprinted from Taylor) at random, and had the cases that occurred in the next few pages, where the log sine was not the sum of the log cosine and log tangent, noted down.* The result was that the numbers of "disagreements" in each minute for the 20′ between 21° 15′ and 21° 34′ (inclusive) were 15, 13, 16, 11, 19, 20, 13, 13, 27, 14, 15, 13, 18, 18, 15, 13, 14, 7, 11, 15; thus there are 300 disagreements out of 1200 cases, or the ratio is exactly $\frac{1}{4}$ (the *exactness* being merely a coincidence, of course; it is curious that Gauss met with a similar coincidence in a trial of the same kind that he made with 900 arguments). It would be worth while (and I hope to be able to have it done at some future time) to form a similar ex-

* In making an examination of this kind care must be taken to omit the bottom line of each page (or to allow for it if included), as it is repeated on the next page: $x°\ 60′=(x+1)°$.

amination for the whole of the canon as it appears in Shortrede, not with a view of verifying the result given by the theory of probabilities, but to examine the different results that might appear to follow by taking separately different portions of the table, &c. Of course the theory of probability *must* be right, and if a series of facts do not agree with it, it is merely so much the worse for the latter; or in more accurate language, it merely shows that the practical case is not of the kind contemplated in the theory, so that in strictness the latter can never be *verified*. But what is of interest in a series of facts to which the theory does apply, is to watch the manner in which the absolute deviations from the law may increase, while the relative deviations decrease. One usually has recourse to tosses of coins, throws of dice, &c., for such illustrations, but the comparison just suggested would present the great advantage of enabling any one to examine the "runs of luck," &c., which would be always open to verification, while in the former cases they must be received on the *ipse vidi* of the experimenter who might have been mistaken.

In relation to a subject on which I commented at some length in my last communication to the Society on logarithmic tables (printed in the *Monthly Notices* for March, 1873), I wish to call attention to a remarkable paper by A. Gernerth, entitled 'Bemerkungen über ältere und neuere mathematische Tafeln,' Vienna, 1863 (reprinted from the *Zeitschrift f. d. österr Gymn. Heft* vi. p. 407), from the contents of which the views I then expressed on the necessity for some body exercising a continual revision over tables receives a most remarkable confirmation. My attention was called to this tract by Professor Bierens de Haan, in consequence of a paper of mine in the *Philosophical Magazine* on a cognate subject, and although a copy of it was lying on the table before me at the time I was correcting the proof-sheets of my last paper, I had not had the opportunity to do more than glance at its contents, else I would have added a note containing the remarks I now place here. The author has examined a great number of tables (by eighteen authors), with the view of testing their accuracy, but he has confined himself to the determination of the correctness of the last figure only. This he has done advisedly, rightly considering that the attention paid to secure accuracy on this point properly represents the care and competency of the editor; although his results would have been of more practical value if he had examined all the figures, as in point of fact no one relies on a result given by means of logarithmic tables as really accurate, it being well known that errors of even two or three units in the last place may occur, so that a last-figure error of unity is *practically* to the user of a table of not much importance. Still, as Herr Gernerth has remarked, the last figure must be printed, and so it had better be printed correctly, at all events, when an ordinary amount of care on the editor's part would ensure such being the case. Any one, however, who will look at Herr Gernerth's paper will be astonished at the extraordinary

amount of carelessness shown in this respect; for instance, in Gronwaldt's six-figure chord table (Quedlinburg, 1850), in 1920 tabular results, there appear 648 such errors; in Rühlmann's six-figure logarithmic and trigonometrical tables (Leipzig, 1859) 1493 errors were found in 31680 tabular results, and so on; all of which ordinary care would have avoided. In Domke's nautical, &c., tables (six figures) 2·58 of the tabular results examined were found to be inaccurate in the last figure; Hülsse's Vega (1849) has 5·56 per cent thus erroneous. And these are far from being the worst (Beskiba's *Lehrbuch für . . . Arithmetik* occupies this position, having 70·28 per cent of the results incorrect, but this is seen to be due to a constant error.) The tract in no way occupies the ground traversed by my paper, being in fact complementary to it; for while I was concerned merely with a certain class of hereditary errors occurring in the logarithms of numbers, Herr Gernerth considers only such as are due to very different causes, and most of his results have reference to the logarithmic trigonometrical canon. The conclusion, however, that he draws relative to the exceedingly unsatisfactory way in which mathematical tables are edited affords a striking confirmation of what I found; and while my investigation showed rather the incompetence and want of knowledge of the average run of editors, he points out forcibly their carelessness; and, what is worse (with the exception of Schrön's beautiful tables and a few others), there is very slight tendency exhibited in the direction of a higher degree of accuracy. With regard to the per-centage table on p. 39 of Herr Gernerth's tract, it is especially to be noticed that they only have reference to the errors found by himself, and not to the total number known to exist: thus, if his examination gave 100 errors, of which 50 were known previously, he would only in forming the per-centage for that table have treated it as containing 50 errors. This mode of formation deprives the list of much of the interest which otherwise it would have possessed in a high degree, as the point with which mathematicians and users of tables are concerned is the number of errors that any particular work contains, not merely the number for the discovery of which we are indebted to Herr Gernerth. In the tract the author adverts to the care taken in correcting the succeeding editions of Callet, which seems to have been very much less than I without examination had imagined (*Monthly Notices*, March, 1873). It is proper also to state that he gives a long list of 598 errors that he has detected in the *Opus Palatinum*, and of 88 errors additional to those given by Vega, in the *Thesaurus Mathematicus* of 1613.

With regard to what constitutes an error, I cannot quite agree to Gauss's definition that any deviation, however slight, from the highest attainable accuracy should be called an error, and the author reprobated accordingly. In the *Monthly Notices* for May, 1872, I remarked that, supposing the seventh, eighth, ninth, and tenth figures of a number were, say 6499, it seemed to me that the seventh figure contracted was with equal accuracy,

as far as the user of the table was concerned, either 6 or 7, and that no computer should be expected in a case of this kind to continue his calculation to more places for the purpose of making a decision according to an ideal standard of accuracy, nor need we feel any gratitude to any one who did take such trouble. In fact, the proper standard ought to be, I think, that the tabular results should never be erroneous by more than ·555... of a unit in the last figure, and that this should be the criterion adopted. However, in contracting a ten-figure table to seven figures, or in any other case, the nearest approach to the truth should invariably be given. The last figure, as before remarked, must be printed, and so it may as well be as near the truth as possible, viz. an editor should not be required to take any trouble to reduce the error from ·555... to ·500...; but, if it is no more trouble to him to so reduce it than not to do so, the more accurate value should be given. In fact, there is *some* reason for the greater accuracy, but it is so small that almost any argument on the other side drawn from convenience would outweigh it. As might be gathered from my remarks about Vlacq, it seems to me that an author who explains his method fully, and the steps he took to ensure accuracy, accomplishes his part of the work, the errors that arise (except such as are due to carelessness) being merely instances of want of completeness (which a succeeding computer may perfect), and not errors in the sense of conveying reproach. I ought also to mention on the subject of logarithmic tables Professor Bierens de Haan's *Iets over Logarithmentafels,* Amsterdam, 1862, 8vo. (reprinted from the *Mededeelingen der Koninklijke Akademie van Wetenschappen, Afdeeling Natuurkunde*, Deel. xiv), which contains an extensive bibliography of the subject.

I here append a letter relating to my two communications in the May and June numbers of the *Monthly Notices* of last year, which I had much pleasure in receiving from Mr. J. N. Lewis, of Mount Vernon, Ohio.

"Dear Sir,—I have lately received Vol. xxxii. of the Royal Astronomical Society's *Monthly Notices*, and have read with much interest your articles (pp. 255 and 288) 'On Errors in Vlacq's Tables of Ten-figure Logarithms of Numbers.' Having myself collected together some of the most noted tables of logarithms—a few of them old ones—I occasionally spend a leisure hour in looking over them, and making here and there a note. I have a copy of the 'Miller' edition of Vlacq, and just before receiving the volume of the *Monthly Notices*, containing your articles, had copied from it and sent to the officers of our Coast Survey a small list of corrections not contained in the list of errata by Vlacq himself, nor in those of Sherwin and Lefort. I found by the reply, however, that our Coast Survey Office does not possess a copy of Vlacq. In the Congressional Library there is a copy of the *Trigonometria Artificialis,* but none of the *Arithmetica Logarithmica.* And here I would remark that your

note (bottom of p. 257) that 'this list' (of errata) 'does not occur in Miller's copies,' is not entirely correct. My copy is one of Miller's, 'Printed *by* George Miller,' not '*for* George Miller.' as your article reads, p. 256, line 2; and it contains the errata table, headed 'Faults escaped amend thus.' Sherwin says, 'Vlacq's own Errata Table is found in few of his books,' but he makes no distinction between the Latin and English copies.* Indeed, he makes no allusion whatever to the English copies. It would seem, then, that the errata table was wanting in some of the Latin copies, as well as the English. The number of printed errata in my copy is 120 (in 3 columns), as stated by you. This copy contains the autograph of Michael Taylor, and, as is clear from manuscript notes, has been used by him, or some other person, in preparing 7-figure tables. The corrections (with a pen) seem to be in an older hand than Taylor's, and may possibly have been made by Gardiner, who says (preface to his Tables) 'I likewise examined and corrected Vlacq's 100 chiliads of logarithms, and their differences, with the utmost attention.' However this may be, the corrections seem to have been made with the greatest care. Something like a year ago I compared them with M. Lefort's table (*An. de l'Obser. Imp. de Paris*, t. iv. pp. [148] et seq.), and found that *every* important error noted by Lefort had been discovered and corrected, and a great part of the unimportant ones also—unity in the 10th decimal place—so that my copy of Vlacq has been much more carefully corrected (by Taylor, or Gardiner, or some other former owner) than Maskelyne's, in which you say, p. 259, that 265 of Lefort's errors are not corrected, 43 of which are serious. I find, too, that *all* those in your table (*Monthly Notices*, xxxii. p. 258) have been discovered and corrected in this old volume. Even the logs of 11275 and 54040 are corrected. I further find, Diff. to log of 9552, for 45639, read 54639. This correction is not in your table, nor any other that I have seen. The log of 26613 has the 8th decimal (3) made with a pen. The same is the case with the 8th decimal (6) of the log of 33509, the 7th decimal (6) of the log of 40217, the 6th decimal (3) of the log of 56359, and the 3rd decimal (9) of the log of 69163; but whether these were defectively printed figures, afterwards amended with a pen, or not, I cannot tell. The log of 57756, noted as erroneous by Sherwin, is correct in my copy of Vlacq. I have examined the 25 numbers given by you at the bottom of p. 260 as imperfectly printed, and find the following to be the case in my copy:—576 is nearly perfect; 4576, the same; 7106, the 6 somewhat imperfect—no danger of mistaking it; 16826, nearly perfect; 19650, the 9 rather imperfect; 21286, perfect; 24077, the 4 rather imperfect—no danger of mistake; 30420, the last 0 a little imperfect; 31176, the 6 rather imperfect; 31226, the 6 imperfect (dim); 33326, the 6

* Some copies of the *Arithmetica* of Vlacq had the explanations in French. See *Advertisement* to Callet's Tables; Thomson's *History of the Royal Society* p. 262, &c.

somewhat imperfect; 37088, nearly perfect; 41426, perfect; 45876, perfect; 61226, the last 6 imperfect (dim); 61526, the last 6 imperfect; 66876, perfect; 66896, the last 6 imperfect; 81026, the last 6 imperfect; 83864, the 6 nearly perfect; 96903, the 0 imperfect; 97318, the 8 is dim (also the last figures of 97319 and 97320); 97326, nearly perfect; 97328, the 8 rather dim—no danger of mistake; 98280, the 0 imperfect.*

"In noticing M. Lefort's correction to the log of 53053, you say 'the correction probably has reference to some other number.' I found, some time ago, that this correction most probably had reference to the log of 53050. M. Lefort's errata table requires also the following corrections, not noted by you:—At Number 8155, in col. of errors, for 3·22, read 5·22; and at N. 39626, for · read 2. (I think the · in M. Lefort's copy of Vlacq must have been caused by a defect in printing, for mine has 2, as it should have.) M. Lefort notes with an asterisk such errata as are common to Vlacq and to the *Thesaurus* of Vega, but he nowhere informs us whether there may not be errors in Vega which are not to be found in Vlacq. In fact, he does not appear to have examined the *Thesaurus* any farther than to compare it with the errata found in Vlacq. I think this is to be regretted, for Vega's book is considerably used at the present day, whereas Vlacq's is no longer procurable. You say, however (p. 262), that Vega's work is scarcer in England than Vlacq's; but I found no difficulty in procuring a copy (from Germany). It seems to me, therefore, that M. Lefort, or M. Houel, or Dr. Bremiker, or some other patient editor, would be doing the mathematical world a service by making a searching and critical examination of Vega's *Thesaurus*, both as to the Numbers and the Trigonometrical Canon. In the latter, Dr. Bremiker says the uncertainty in the last figure amounts to 4 units. (For a few corrections of this Canon see the tables of Hobert and Ideler, p. 350).

"At p. 260 you give a list of errors in the column of 'Num.' in Vlacq's *Arithmetica*, which have not heretofore been noticed. To this list I would add the Num. 347, and here I would say that I cannot be sure that my copy of Vlacq (or Miller) does not contain other manuscript corrections not heretofore noticed by the editors of later tables, nor contained in any of the tables of errata you have examined, for I have only given the matter a few hours' examination, for the purpose of sending a few corrections to the officers of our Coast Survey, as stated above.

"You say, p. 257, 'In 1631 Vlacq published his *Trigonometria Artificialis*.' I have not a copy of this book, but I have never elsewhere seen the date of publication other than 1633. Allow me to correct two or three other small oversights:—p. 261, lines 7 and 6 from bottom, for 'seventh, eighth, and ninth,' read

* [The letter contains sketches showing the shape of most of the figures noted as imperfect.]

'eighth, ninth, and tenth;' p. 262, middle of page, for 'Adrian Vlacq,' read 'Adriani Vlacci;' and for '305 pp.' read '307 pp.' (The logs occupy pp. 3 to 309, both inclusive;*) p. 288 I find the following: 'It does not follow that the number of errors found by Vega in 452—301=151,' &c. Should not this read, 'It does not follow that the number of errors *not* found *in* Vega *is* 452—301=151?' &c. On the same page, bottom, the correction of log of 64818 is given. This had been noticed by you before, p. 258. I may note here on the matter you refer to on p. 289, that my copy of the *Thesaurus* of Vega has only *one* page of errata, p. xxx. of the Introduction, the same errata being repeated, however, in a little different form on p. 685.)

"I see you refer (note, p. 259) to the question of the date of the 2nd edition of Sherwin's *Mathematical Tables*. I have four editions of Sherwin, the title-pages of which bear the dates 1717, 1742, 1761, and 1771. The last three are called the 3rd, 4th, and 5th editions, and that of 1717 I consider the 1st edition, with a new title-page added, a thing not uncommon, I believe, in those days (nor at the present time, either), when part of an edition remained unsold for some years. There are some reasons which induce me to think that nothing but the title-page has been changed, among which are—1st, there is nothing said anywhere of its being a second edition; 2nd, there was certainly an edition (the 2nd, I believe) issued in 1726; 3rd, this 1st edition (as I call it) contains Sharp's logarithms to 61 decimals for all numbers under 100 and of all primes under 200 (some of them, however, short of 61 by 2 to 10 decimal places). Now, Sharp's *Geometry Improved* appeared the same year, 1717, of the date of the title of this edition of Sherwin, and it (the *Geometry Improved*) contains the logarithms of all numbers to 100, and of all primes under 1100 to 61 decimals. If, then, this edition of Sherwin was really of the date 1717, why should it not contain these additional logarithms of Sharp, as subsequent editions of Sherwin do? Again, Gardiner, who edited the 3rd edition, so called, certainly knew how many editions had gone before; and further, the copy I have, with date 1717 in the title, contains a second title-page, at the beginning of the tables: 'Mathematical Tables, London: printed by *S. Bridge*, for *Jer. Seller* and *Cha. Price*, at Hermitage Stairs, in Wapping; and *John Senex*, next Door to the *Fleece Tavern*, in Cornhill, 1705.' This last date is, I have no doubt, that of the 1st edition, though Hutton, and others following him, make it 1706, for what reason I do not know. All agree that the dedication is dated July 12, 1705. Dr. Bremiker gives 1705 as the date of the 1st edition, and so does Lalande, Astron. (3rd edition) § 4104; but in his *Bibliographie Astronomique*, p. 366, gives 1706, when also he gives the dates of other editions, 1717, 1726, 1741, 1742, 1761, and

* Same place, for 100,100, read 101,000.

1771. I know nothing of an edition in 1741, though Callet gives that date also, as well as 1724. Hobert and Ideler (see *Neue Trigonometrische Tafeln, Einleitung*, p. xliii) give 1741 as the date of the 3rd edition, and it seems certain there were copies bearing this date. See also Thomson's *History of the Royal Society*, p. 262.* Barlow, *Mathematical Dictionary*, gives both 1704 and 1706, *in the same paragraph*, as dates of the 1st edition; but there are so many errors in the article 'Logarithms' in this Dictionary, that it is useless to quote from it. Delambre (*History of Modern Astronomy*, vol. ii. p. 90) says he possessed the editions of 1717 and 1726. He gives the date of the 1st edition, 1706, only as a conjecture of Lalande.

"What little examination I have given to logarithmic tables has been mainly induced by reading Professor De Morgan's very full and interesting catalogues of such tables in the *Penny Cyclopædia* and the *English Cyclopædia*. I have found Professor De Morgan's descriptions of tables to be, in general, quite correct—so far as I have been able to verify them—yet, at the same time, he has committed a few oversights. I will give only one example. He says Delambre is wrong in saying that the tables of Hobert and Ideler (Berlin, 1799) subdivide the quadrant as minutely as those which himself and Borda published; which latter he (De Morgan) describes as follows:—every 1″ from 0 to 3°, every 10″ from 3° to 40°, every 1′ from 40° to 50°. Now, if dividing the quadrant to a certain degree of minuteness means that, to that degree of minuteness, the logs of the functions can be read out of the tables immediately, and without adding proportional parts, then the above description is wrong, though it is called a correction of a former description given in the *Penny Cyclopædia*, article 'Tables.' The correct description is as follows:—

"To every 10″ from 0 to 3°, with proportional parts of differences for each single second.

"To every 1′ from 3° to 50°, with proportional parts of differences for each 10″.

"There is absolutely no difference between the 40° to 50° part of the table, and the 3° to 40° part, except that in the former the proportional parts are arranged differently, and given for each 5′ only, so as to economise room. Delambre's description is correct. See his *History of Modern Astronomy*, vol. i. p. 556; also same vol., p. 557, and his preface to Borda's Tables, p. 113, where he says Hobert and Ideler's Tables are of the same extent† as Borda's. In this matter—of the minuteness with which Borda's Tables subdivide the quadrant, as compared with Hobert and Ideler's—even the careful Dr. Bremiker (see his preface to

* Where, with the date 1706, the title of *Gardiner's* tables is given, not *Sherwin's*.

† Hobert and Ideler's Tables have no proportional parts, otherwise they (in the trigonometrical parts) are the same as Borda's.

Vega's 7-figure logarithm, p. iv.) has fallen into the same error as Prof. de Morgan; perhaps has copied from the latter.

"I have some other notes on logarithmic tables, but the above must suffice for the present.

J. N. Lewis.

Mount Vernon, Ohio, U.S.A.
March 12, 1873."

In reference to the contents of this letter I have to remark that copies of the (Miller) Vlacq of 1631 having an errata list must be very rare. I have carefully examined four Miller copies, and none of them contain any such list, or any reference to one; and as remarked in my last communication to the Society on the subject of logarithmic tables, it is clear that John Newton in 1658 reprinted from a copy without an errata list. Vlacq, in his work of 1628, not only gives the list of errata, but refers to it in his preface, so that I imagine all the Latin copies have the list, but in none of the Miller copies I have seen does Vlacq's preface appear. The existence of the French edition I alluded to in a postscript to my last paper (and previously in the *Phil. Mag.*), and I there also noticed the confirmation of my opinion that there was also a Dutch edition that was afforded by a statement I quoted from a letter of Briggs. I have since written to Professor Bierens de Haan at Leyden, and asked him to see if there exists any such copy in any of the libraries there, but he informs me that he can find no trace of any edition having appeared with the introduction in Dutch. I still, however, think it likely that the tabular portions of the Miller copies were originally intended to form part of a Dutch edition.

The knowledge of the whereabouts of Michael Taylor's copy of Vlacq is interesting, and it is valuable to know that all the important errors found by means of the French manuscript tables were known to Taylor. In my last paper I showed that every error that can affect a seven-figure table of the logarithms of numbers had been published by 1794, and Mr. Lewis finds that every important error (*i. e.* every error greater than a unit in the last figure) had been discovered before that date, though of course it does not follow that they had all been published, for I believe that neither Gardiner nor Taylor gave publicity to nearly all the errors they discovered. The error in the Diff. to log 9552 is pointed out by Vlacq in his own errata list. With regard to the alterations which Mr. Lewis considers to have been made by the pen, I find by examining the logarithms in question in three copies (one of 1628 and two of 1631) now open before me, that the logs of 26613, 33509, and 69163 are correctly and clearly printed in all three; that the top of the 6 in log of 40217 is out of its place in all three, the type having been damaged; that in the log of 26613 the 3 is exceedingly faint in the two Miller copies, in which also the lower part of the 3 in the log of 56359 is blotchy; none have been touched with the pen, and the faint 3

in the log of 26613 is the only one requiring improvement. From the Latin copy being the best it may be inferred that the tables for the 1628 edition were printed off before those that were subsequently used in the Miller copies.

With regard to M. Lefort's erratum in log of 39626, I have the 2 in all stages, in one copy (1628) it is printed quite clearly, in another with the upper part illegible, and in the third it appears only as a point. All the oversights that Mr. Lewis kindly points out as occurring in my paper are just, including "*by* George Miller" for "*for* George Miller." The date of the *Artificialis* is 1633.

The "number of errors found by Vega in 452—301=151" conveys a meaning exactly opposite to what was intended through a misprint. I wrote *is*, not *in*, and when this correction is made, the remark is to the effect that although Vlacq contains 452 errors and Vega 301, it does not follow that Vlacq detected and corrected 151 of the former (as the two tables are not of equal extent), so that my meaning was the same as that suggested by Mr. Lewis. I entered into so much detail about Sherwin's tables in my last paper that it is scarcely necessary to add anything further here. It will be seen that all the editions noticed by Mr. Lewis had come under my own eye. The existence of copies with the date of 1706 is certain. One is before me now, and I think I have seen two copies with this date, and two with that of 1705, so that as far as this is any guide, the numbers of each may be about equal. Though I do not think the argument derived from the logarithms in Sharp's *Geometry Improv'd* is of much weight, still it seems most probable that the first impression formed the copies of 1705, 1706, and 1717.

Having had occasion to examine a very great number of the works noticed by De Morgan in his article in the *English Cyclopædia*, I can most heartily endorse Mr. Lewis's opinion of the accuracy of the descriptions contained therein; and only those who have tried know the extreme amount of care that is required in order to describe truly the contents of a number of tables, with their different arrangements. In De Morgan's article the notices of the tables are too brief to be very useful, but their closeness to the truth is admirable. Curiously enough, in describing Borda and Delambre for the report of the British Association Tables Committee I fell into the same mistake that De Morgan did (and as I now learn for the first time from Mr. Lewis that Bremiker did also), and only discovered the error in a subsequent revision, when I noticed a trifling inconsistency, to correct which rendered it necessary to look at the work itself again. The circumstance afforded me some uneasiness at the time, when I considered that but for an accident it might have appeared that the general statement prefixed to the Report to the effect that the descriptions had been made solely from inspection of the works themselves was subject to exception. It must be confessed that the part of the table beyond 10′ is a trap in which any one used to

describing tables would be most likely to be caught, as the arrangement is so exactly similar to what it would usually be, if the contents were as described by De Morgan, that to all who had not absolutely used it such would seem to be the case. It is not easy at first sight to see why the log sines and tangents were not given to seconds, &c., instead of the room they would have occupied being devoted to the proportional parts; but the explanation of course is that by the actual arrangement the log sines and cosecants, tangents and cotangents, &c., are placed on an equality, as the proportional parts are the same for both. It does not seem very clear that the choice was a wise one.

I will conclude with the remark that the fact of my former papers having elicited the above interesting letter from Mr. Lewis, affords a justification for my having made these communications to the Society in an avowedly imperfect state, without delaying their publication till I was satisfied that they were as complete as I could make them. It is only by the co-operation of several workers, possessing different sources of information, that accuracy or high excellence in this direction is attainable.

Cambridge, April 11th, 1873.

Postscript.—Mr. E. A. Hadley has pointed out to me that the statement on p. 337 of my paper *On the Progress to Accuracy of Logarithmic Tables* in the March *Notice*, that 'the two errors which, though discovered by 1794, exhibit such a persistent vitality, are those in log 38962 and log 52943, and [that] they occur in all the tables subsequent to 1827, where two or more are assigned in the list,' is not quite accurate. In fact, log 38962 is correctly given in Hassler, 1830, the tables published in Chambers' Educational Course, 1847 and 1853, and Callet, 1855. It is however quite true that the errors in the two logarithms in question are the most persistent, in the sense that they have survived the longest, as they both appear in Sang, 1871. On referring to the original comparisons whence the list on p. 335 was compiled, I find that of the thirty-one works (leaving out Vlacq itself) included therein, log 52943 is inaccurate in 25; log 57628, log 57629, and log 63747, in 17; log 38962 and log 67951 in 16; log 53919 in 15; log 24626 in 14; log 33071 in 13; log 81674 in 12; log 60844 in 9; &c. Thus log 52943 is *facile princeps*, and the only tables (leaving out of considerations the four that are quite free from error) in which it does not occur are the Vegas of 1794 and 1797.

May 21st, 1873.

Ephemeris of Tempel's Comet. By J. R. Hind, Esq.

The following positions are derived from Dr. von Asten's orbit for 1873, with the time of perihelion passage assumed May 8·704 G.M.T. On this assumption the correction to the calculated place, by the Marseilles observation of May 1, was + 19ˢ·0 in R.A. and −10′ 22″ in N.P.D.

At Greenwich, Midnight.

	R.A.	N.P.D.	Log. Δ	$\frac{1}{r^2\Delta^2}$
	h m s	° ′		
May 16	16 32 54	105 29·0	9·89050	0·525
18	31 50	105 48·2	9·88867	
20	30 43	106 7·7	9·88728	0·532
22	29 33	106 27·6	9·88634	
24	28 20	106 48·6	9·88586	0·534
26	27 5	107 8·7	9·88585	
28	25 48	107 29·5	9·88630	0·531
30	24 32	107 50·6	9·88722	
June 1	23 16	108 11·9	9·88861	0·524
3	22 2	108 33·3	8·89046	
5	16 20 50	108 54·7	9·89274	0·511

With the above time of perihelion passage in Dr. von Asten's ellipse, the geometric longitude at M. Stephan's first observation (April 1) is exactly represented, a difference of about 4′ remaining between the observed and computed latitudes.

Observation of Tempel's Comet of Short Period.
By M. Stephan, Director of the Observatory at Marseilles.

(*Communicated in a Letter to Mr. Hind.*)

May 1 at 13ʰ 17ᵐ 24ˢ Mean Time at Marseilles; New Observatory.
R.A. 16ʰ 37ᵐ 51ˢ·59 N.P.D. 103° 11′ 52″·3.

The correction for parallax is not applied.

Mean Position of Comparison Star, Weisse's 738 H. XVI. for 1873·0.
R.A. 16ʰ 39ᵐ 39ˢ·59 N.P.D. 103° 6′ 19″·2 Lal. 1 Bessel 2.

The comet was still very faint, though somewhat less so than on April 3.

Elements of Orbit of Σ 1938 (μ^2 *Boötis*).

By J. M. Wilson, M.A.

By graphical processes I obtain the following elements:—

e = ·51	ϖ = 186° 30′
Ω = 172° 0′	P = 200·4 years.
D = 45 0	t = 1865·2
λ = 20 5	

Its present position and distance by our observations are 152° and 0″·6 nearly.

Temple Observatory, Rugby, May 1873.

Position of the Components of ξ *Ursæ Majoris.*

By Wentworth Erck, Esq.

April 10, 1873.

Mean of 20 measures, whereof 10 were just previous to Meridian Passage, and 10 just after = 0°·4

April 11, 1873.

Mean of 20, taken as before .. = 359 ·9

Epoch 1873·101; Mean Position .. = 0 ·2

Estimated Distance 0″·9

The above were taken with the 7½-inch Alvan Clarke, and power 750.

Sherrington, Bray, April 12, 1873.

Jupiter's Fourth Satellite on and off the Disk.

By E. Firmstone, Esq.

I observe in the *Monthly Notices* a letter from Mr. Roberts respecting the dark transit of *Jupiter's* fourth satellite on the evening of March 26. I observed the same with my small telescope, 2¾-in. aperture, and, the weather being clear here, saw the emersion also. The satellite was perfectly *distinct*, though of a darker or more leaden shade than the others. Of course with my small glass I could not see the deficient segments of the dark disk, of which Mr. Roberts speaks. While I write, another of these dark transits is in progress. The satellite is much more plainly visible, and apparently fully as dark in view, than any "shadow" except that of III. I shall watch for the emersion, and do not doubt that the satellite will be seen at once.

Winchester, May 15.

Errata in Mr. Burnham's Catalogue of New Double Stars.

Page 352 (note to No. 5), *for* "in the same field *n. p. Piscium*," *read* "in the same field *n. p.* 105 *Piscium*."

Page 356 (note to No. 67), *for* "1½° *n* of O Σ 415," *read* "½° *n* of O Σ 415."